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# The New York Academy of Medicine 1847-1947

IAGO GALDSTON*

THE New York Academy of Medicine was founded one hundred years ago. It came into being in a decade that witnessed the birth of many scientific organizations destined to have a profound influence upon the peoples of this nation and of the world. It was born when the world was literally in travail.

The decade from 1840 to 1850 is in many ways reminiscent of the current period. Europe was in a turmoil, and the domestic scene was turbulent. The world was still sensible to the effects of the French Revolution and the Napoleonic Dictatorship. Time had not yet dissipated the dread of invasion from the hearts of the English. Many were the lands and the peoples who, in the inspiration of the revolutionary ideals —Liberté, Egalité, Fraternité—sought to win political and social independence, and attempted to throw off the yoke of imperial dominance.

The world at this time was keenly and painfully aware of great prospects. Every realm of culture and science was quickened with the promise of large and revolutionary achievements. The air was vibrant with expectancy, and men fostered large visions with the uncommon faith that they would certainly be realized.

This was the decade in which Ralph Waldo Emerson published his *Essays,* and Jacob Henle his postulates on Miasmata and Contagia. In this decennium Texas was annexed to the United States; Morton introduced ether anesthesia; and the Smithsonian Institution was founded. Thackeray wrote *Vanity Fair,* and John Stuart Mill his *Political Economy.*

The fifth decade of the last century witnessed the revelation of a large number of fundamental scientific data upon which the succeeding decades built the magnificent edifice of modern science. Schleiden and Schwann had just formulated and published their treatise on the cell theory. It was during these years that Claude Bernard discovered the digestive function of the pancreas and the glycogenic function of the liver, and Helmholtz published his treatise on the Conservation of Ener-

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gy, located the source of animal heat in the muscles, invented the ophthalmoscope, and estimated the velocity of nerve current.

These, too, were the times of heroic first ventures. The American scene had been brightened by the courageous Ephraim McDowell, a master surgeon in the wilds of Kentucky; by William Beaumont who made two hundred and thirty experiments and then published at his own expense the classic volume entitled: *Experiments and Observations on the Gastric Juice and the Physiology of Digestion;* by Samuel David Gross, the scholarly surgeon and anatomist; and by James Marion Sims, the Nestor of gynecology.

In the early decades of the last century New York was a motley city. For many years it had trailed both Philadelphia and Boston in the race for cultural and medical leadership. The early medical pioneers were mostly either Philadelphians or Bostonians.

There is a valid explanation for this. New York was from the very beginning a cosmopolitan center, complex in character, and heterogeneous in population. It did not jell as early as did either Boston or Philadelphia. Withal, New York was not without medical distinction even in its earliest days. Nor was it without priorities in medical achievements. John Jones, to whom American surgeons must look back as the first of their eminent professional forbears, was a New Yorker. Jones served as a surgeon in the Revolutionary War, and was the author of the first American book on military medicine,

John Bard and his son Samuel, though Philadelphians by birth, were New Yorkers throughout their professional careers. The elder Bard came to New York on the advice of Benjamin Franklin. Together with Dr. Peter Middleton, he offered the first formal instruction in medicine, this being a course in human anatomy by the dissection of the human body. Samuel Bard, the son of John Bard, was considered the most eminent American physician of his time with the single exception of Benjamin Rush. Despite the loyalist sympathies he manifested during the Revolutionary War, Samuel Bard was chosen to serve as Washington's physician when the latter became President. Samuel Bard organized the first medical school in the city of New York. The school foundered in the Revolution. He was also instrumental in organizing New York Hospital.

New York was the first to regulate the practice of medicine and surgery by an act of the Lieutenant Governor, the Council, and the Gen-

eral Assembly (1760). New York can likewise claim the distinction of having had the first medical society. It was called the Weekly Society of Gentlemen. This society was merged with the New York State Medical Society organized on November 14, 1794. In 1806 the society changed its name to the New York County Medical Society.

Despite the many eminently competent men who practiced medicine and surgery in New York City, and despite the sporadic efforts that were made to develop the medical facilities of the city, New York did not achieve a significant place in American medicine until about the middle of the last century. In 1840 the city was adolescent and clearly foreshadowed its future character. It had grown to be a populous city, busied with many enterprises and industries. It was not, however, a beautiful city, nor was it healthful. All these factors served to stimulate, and provided opportunities for, medical progress. The need arising, man rises to the need. New York was ravaged by epidemic and endemic diseases. Its mortality rate during the first decades of the nineteenth century was approximately 40 per thousand. During this time we find the New York State Medical Society offering prizes for essays on such significant subjects as Tuberculosis, its Remote and Proximate Causes; The Causes and Treatment of Typhus Fever; and, properly enough, The Treatment of Delirium Tremens.

One of the prize essays honored by the Society dealt with the subject of industrial hygiene. It was written in 1837 by Benjamin W. McCready, and was entitled "The Influence of Trades, Professions, and Occupations in the United States in the Production of Disease." Dr. McCready gained most of his experience in industrial medicine in New York City.

The widespread prevalence of disease, the poverty and degradation in which so many of the poor and the immigrants lived, the general filth and noise, the plague of quacks that afflicted the city, set many earnest and philanthropic persons to move in their respective ways toward the common goal of improving the lot of the common man, and of redeeming and improving the city. In this march forward, which began early but gained full stride only after the Civil War, can be counted many sectors. New medical societies were formed, new hospitals built, and old ones revamped; medical specialties had their beginnings then. The older medical schools vastly improved their teaching, and new commercial schools came into being.

Medicine, as has been observed, had sunk to low levels during the

early decades of the nineteenth century. Quackery was rampant. Fly-by-night "medical schools" let loose upon the public many badly trained and unscrupulous practitioners. The law provided no remedy. The qualified physicians of the city were but loosely organized in small neighborhood clubs. The profession was without the means and the opportunity to meet in common and to share its varied experiences. The profession had no regular meeting place, and no effective, readily available medical library.

It was at such a time and upon such a world scene that The New York Academy of Medicine came into being. We do not know how much the founders of the Academy were influenced in their ambitious undertaking by the temper and tide of the times. Their early pronouncements reflect a rather parochial and sober view, seemingly unaffected by global considerations. Yet it is not likely that they were untouched by the spirit of their time. One thing is certain: They had a high *moral* sense of the place and function of medicine in society, and they gave to that ideal first place among the motives for the creation of the Academy.

To elevate the character of the profession, to advance its interests, and to increase its usefulness, by "furnishing facilities for medical intercourse, promoting harmony among its members, and offering means of mutual improvement therein" were the avowed objectives, for the attainment of which all *regular* practitioners of the city were invited to convene on Saturday, December 12, 1846, at 1. P.M. at the Lyceum of Natural History, 561 Broadway. This appeal to the regular practitioners was issued in the names of Doctors Valentine Mott, Alex H. Stevens and Isaac Wood. Doctors Mott and Stevens were presidents of New York University Medical College and the College of Physicians and Surgeons respectively, and Dr. Wood of the Medical Society of the City and County of New York.

The Academy was not actually founded until January 6, 1847, and the professed objectives of the Academy were rephrased from time to time. Yet basically they have not changed since the Academy was first conceived, and whatever variations may be perceived are to be considered added niceties of expression and timely shifts in emphasis.

It has always been the avowed aim of the Academy to foster the best ethical relations between the profession and the public, and within the profession itself. It has always been the aim of the Academy to provide the best possible opportunities for the continued education of the phy-

sician and for the encouragement and promotion of the advancement of the medical science in all its numerous parts and phases. The Academy has ever been concerned with the public health. And the Academy, despite many and in some instances rather amusing skirmishes with the press, has consistently promoted the education of the public in all matters that pertain to disease prevention and the promotion of good health.

In the long perspective of a hundred years it can now be seen that though the Academy owes much to the men who guided its destinies, it owes its survival and its great growth to the soundness, to the pertinence, and to the social merit of its basic objectives. The founders of the Academy had both courage and vision. But between vision and attainment lies the long stretch of reality and experience, studded everywhere with harsh vicissitudes. The founders not only had courage and vision, but not a few among them appear to have been strong-willed and contentious individuals, with rather short tempers. The Constitution of the Academy was written, then rewritten, then rewritten again. Always some members were delinquent in paying their dues, and the Treasury was constantly on the brink of bankruptcy. The meeting places where the Fellows might gather were poor and inconvenient. Convention Hall, at 175 Wooster Street, where the Academy held its meetings during the first three years, was located over a coalyard. There was continuous and sustained complaint, among the Fellows, and even in the public press, that the Academy's meeting place was "foully ventilated." There was a volunteer librarian, but no library, and such few books as had been gathered were in the keeping, and at the mercy, of the librarian.

Yet, and despite, the Academy survived and grew. For it had a strength of purpose, and an inner rationale, meet to the needs of the times, the profession, and the community. The scientific papers delivered during the first year of the Academy's existence are attestations thereof. Dr. Valentine Mott read a paper on the "Treatment of Ununited Fractures." Dr. Arthur B. Stout delivered an address on the "Revelations of the Microscope." Dr. Pliny Earle, Superintendent of the Bloomingdale Asylum in New York, and a founding member of what today is known as the American Psychiatric Association, delivered "by invitation" a paper entitled "An Historical Sketch of the Institutions for the Insane in the United States of America." Dr. J. P. Batchelder discussed "Symptoms, Phenomena and Exciting Causes of Inflammation." Dr. Chandler R. Gilman read a paper on "Placenta Praevia." The quality of the scientific

papers improved appreciably by 1851. In 1854 the Section meetings were organized.

On the score of its concern with the public health the Academy made no less impressive a showing. At the first of its stated meetings in 1847, the Academy voted to memorialize the State Legislature endorsing a proposed law for the registration of births, deaths, and marriages. During the same year it voted to urge Congress to legislate control over the importation of drugs. It addressed still another memorial to the State Legislature, urging action for the care of youthful idiots. This was certainly a lusty entry into the field of public health.

On November 10, 1847, the Academy held its first Anniversary Meeting. The meeting was at the Broadway Tabenacle. The orator was Dr. John W. Francis and his oration lasted two hours. A contemporary publication, the *Annalist,* reported that "every . . . part of the room was filled, literally filled, to the very walls with well dressed persons of both sexes. Scarcely was standing room afforded either upon the floor or in the galleries. . . . Hundreds must have gone away disappointed being unable to obtain places." The attendance was variously estimated as from 2500 to 4000 and Dr. Francis' oration was well received. There is ample evidence in this that the Academy had gained the sympathetic interest of the public.

The Academy was not loath to make use of the public press, in the public interest. When in June, 1847, it was observed that typhus fever was widely prevalent, the Academy appointed a committee to visit the hospitals, to investigate and to report on the whole subject. The committee did so promptly and the Academy ordered that the report be printed in *the daily press* as well as in the medical journals.

Despite its organizational incertitudes, despite the inner strains and stresses of its body politic, and despite its impoverished treasury, the Academy was yet able, from the first year of its existence on, to elevate the character of the profession and to advance the science of medicine for the greater good of the people.

Only in one respect was the Academy backward in the attainment of its goals. It lacked a library. Shortly after the Academy was founded, Dr. Isaac Wood "started" the library by contributing *Paine's Commentaries* in three volumes. A committee issued a circular soliciting contributions of medical books and other publications. A librarian was elected to have charge of "all books, pamphlets, manuscripts, anatomical

or pathological specimens, apparatus, instruments, medals, coins or scientific property of whatever kind." No provisions were made as to where he was to keep all such items. But evidently he was not put to much trouble on that score, for we find an item in the records of eleven years later, 1858, to the effect that the full cost of moving the *entire* library was but $3.50.

Though the Academy library was founded by Dr. Isaac Wood in 1847, it did not really come into being until almost thirty years later. In 1866 Dr. Samuel S. Purple, a founding member announced his readiness "to donate portions of his medical library to the Academy when it had a permanent home under suitable restrictions for their preservation." To further encourage this project Dr. Purple pledged himself for two thousand dollars towards the construction of the proposed building. It had long been the hope and ambition of the officers and Fellows of the Academy to have a medical home of their own, but the monies required were not available. As far back as 1853 a "Building Fund" had been established. By 1866, the year of Dr. Purple's offer, this Fund amounted to $7,524.22; hardly adequate to purchase a home. Yet, nothing loath, a Committee was appointed in 1866 with "full power to raise funds, select a site, and if necessary, erect a building." Eight years later the Committee attained its objectives and on December 24, 1874, the Academy gained its first home at 12 West 31st Street, a brown stone, high stoop house of four storeys, 28 by 60 feet and on a lot 100 feet deep. Now the Academy could have its library. The two large rooms on the second floor were devoted to housing the library, substantially augmented by Dr. Purple's contribution of 2000 bound volumes of journals and monographs. The library which in 1875 numbered but 400 volumes, in 1879 counted some 9000 volumes. In this year, 1879, the Academy absorbed the Medical Library and Journal Association of New York which had been founded in 1864. This amalgamation made possible the establishment of a circulation division in the Academy library. Still another milestone in the development of the Academy was the opening of the library to the public. In September, 1878, the Academy voted, as recommended by its President, Samuel S. Purple, to admit the public from 10 to 5 daily, and in October this was done. The library has remained open to the public ever since.

With the acquisition of a home of its own, and with the creation of a library worthy of admiration, the Academy literally came of age. As an

institution of learning and of public service it now had attained stature. From this time on its growth was secure and consistent.

If, as was observed before, the survival of the Academy during the precarious years of its youth was assured by the soundness, pertinence, and social merit of its basic objectives, its attainments in the hundred years of its existence are to be credited to the initiative, resourcefulness, and energetic efforts of its Fellows. Among these, however, there is a number whose merits are signal, and to whom belongs far more than a common share of credit. To the founders the Academy owes its inception, and to the first of its presidents, John Stearns, John W. Francis, and Valentine Mott, it is indebted for the skilful piloting of the frail bark through storm and rough sea. Dr. Samuel S. Purple created the Academy's library and advanced its efforts to obtain a permanent home. He also served as President of the Academy for two terms, from 1875 to 1879. He was succeeded by Dr. B. Fordyce Barker, who served for three terms, from 1879 to 1885, and he in turn was succeeded by Dr. Abraham Jacobi, who served from 1885 to 1889. These three incumbents collectively held office for fourteen years.

These were crucial and fateful years in the history of medicine, as well as of the Academy. Most noteworthy was the earnest intensification of the scientific activities of the Academy. In 1879 seventeen papers were read, and in 1880, twenty-two papers, all of them interesting and profitable. The library by 1880 had increased to a total of 17,000 volumes, exclusive of pamphlets. In the fall of that year the Academy listened to a report on the International Medical Congress which had met in London. At this meeting there were introduced and "invited to sit on the platform," Doctors Déclare of Paris, Meyer of Copenhagen, Gelbke of Dresden, Major Gardner, U.S.A., Dedama of Syracuse, Satal of Florida, Newell of New Brunswick, N. J., Shine of St. Augustine, Cushing of San Francisco, and Mussey of Cincinnati. The Academy's meetings were attracting visitors from remote parts of the world.

The Academy in the space of these fourteen years began to outgrow its organizational and physical structure. In his Valedictory address Dr. Barker listed the needs of the Academy under six headings, to wit; a large fireproof building, with ample room for a library to contain the medical literature of the past and of the coming times; a large meeting hall and several smaller ones; room for a museum; an anthropometric laboratory; a large lecture hall connected with a well-equipped laboratory containing

the required appurtenances for demonstrations of every sort; and a paid staff of technical and mechanical experts including a pathologist, a first-class mechanic, and a good photographer.

In presenting these proposals Dr. Barker avowed, "This is a broad scheme which will require a very large endowment, but it is my firm conviction that it only outlines the future of the Academy, even if it demands a million of dollars." This was said in 1885.

The Academy has come a long way in the realization of that foreseen future, and it did so by the indefatigable efforts of those who not only foresaw but who also labored to realize the Academy's future. Most noteworthy among these was the man who succeeded Dr. Barker as President of the Academy, Abraham Jacobi. He was earnestly and passionately devoted to the advancement of medical education and of the Academy's interests. He solicited and obtained money gifts to the Academy. It was during his régime that the Academy received the bequest of $70,000 from the Estate of Celine D. Hosack. This bequest made it possible to undertake a search for the new and larger quarters envisaged by Dr. Barker in his Valedictory address. A successful campaign to raise the supplementary sums required was undertaken by a special Committee, and in December of 1888 the Academy purchased a plot 75′ by 100′ on the north side of 43rd Street, between Fifth and Sixth Avenues. The corner stone of the Academy's new home was laid on October 2, 1889, and the Academy met there for the first time on October 2, 1890. The formal opening of the Academy was celebrated on November 20, 1890, with an impressive programme, which included an Invocation by the Reverend Howard Crosby, D. D.; an address by the President, Alfred Loomis; the Anniversary Oration, delivered by Edward L. Keyes, his subject being The New York Academy of Medicine; and address on "Our Library," by Abraham Jacobi; and a concluding address by Fordyce Barker. The celebration was ended with Addresses of Congratulation by John S. Billings, Librarian, Surgeon-General's Office, Washington, D. C., S. Weir Mitchell, of Philadelphia, Pa., and Reginald H. Fitz, of Boston, Mass.

With the acquisition of its new home the Academy entered also upon its modern era. Time and the advance of modern medicine had paled the significance of some of the objectives listed by its Founders, while heightening the meaning of others. In 1847 it was essential to slough off the *irregulars,* among whom were included homeopathic, hy-

dropathic, chronothermal, and botanic physicians as well as mesmeric and clairvoyant pretenders to the healing art. By 1890, however, though still earnestly concerned with medical education, the Academy was eagerly preoccupied with discussions of the germ theory, with Pasteur's prophylaxis for rabies, with the tubercle bacillus and its relation to the etiology, diagnosis and treatment of tuberculosis; in a word, with the many new, bright, and promising tangents of modern medicine.

To appreciate the growth of medicine in New York City one needs to envisage the phenomenal growth of the city proper. After the Civil War, New York developed rapidly. It became the financial center of the United States. It grew too as an industrial city. New York was the gateway to America, and through it passed the hundreds of thousands of immigrants who for economic, political, or religious reasons had left their homelands to seek refuge in the new land. Most of these immigrants went North, South, and West, but many remained in New York. The city grew rich in all its resources, economic, cultural, and intellectual. Its heterogeneous population, which in former years hampered its integration, now proved an asset. Men from all nations and all quarters of the globe could find a sympathetic reception in the cosmopolis, and brought to it the best heritage of their lands. Among the newly-come were men such as Abraham Jacobi and Arpad Gerster, to name but two, who were destined to become leaders in medicine, and outstanding citizens of the Empire City. It is easy to understand, then, why New York so quickly appreciated and so promptly accepted and fostered the new medical sciences that issued from the labors of Virchow, Pasteur, Koch, and Lister. New York medicine was not far behind the European pioneers. In Francis Delafield, Austin Flint, Janeway, Prudden, Herman Biggs, Park, and Welch, the best of European medicine found worthy protagonists and able disciples. With the turn of the century, medicine in New York became of age. With confidence and competence it assumed its position among the leading medical centers of the world.

Hospital facilities were first available in the City of New York in 1776, when the New York Hospital, chartered five years previously, opened its doors in a provisional structure. Previous to that time, the only so-called hospital to be found on Manhattan Island was the six-bed, one-room infirmary which was housed in the Publick Workhouse and House of Correction of the City of New York. Such municipal care began in New York City in 1736. Out of the six-bed infirmary in the

Workhouse developed the Almshouse Hospital. From the point of view of service, this institution outranks the New York Hospital. In the Almshouse the attending physicians were: Dr. Nicholas Romayne, Dr. William Moore, Dr. Benjamin Kissam, and the surgeon was Dr. Wright Post. This was the first practical establishment as a hospital and dispensary under the corporation of the city which existed in the state. Dr. Nicholas Romayne and the others taught medical students there, and it was he who in 1807 founded the College of Physicians and Surgeons. Furthermore, the first lying-in wards in New York City were established in the Almshouse by Dr. Valentine Seaman in 1799.

Until the middle of the nineteenth century the hospitals of New York were grossly inadequate, and some were indeed disgraceful. Thereafter, the number of hospitals in Greater New York increased rapidly, and there was a remarkable improvement in the quality of their institutional and medical service. Lincoln Hospital and Home, formerly known as the Colored Home and Hospital, was established in 1839. St. Luke's Hospital was opened in 1858; Long Island College Hospital in the same year, and St. Vincent's in 1849. Mt. Sinai, known formerly as the Jews' Hospital in the City of New York, was established in 1852. Roosevelt Hospital was founded in 1863; Lenox Hill (formerly the German Hospital) in 1861, and the Presbyterian Hospital in 1868. Most of the larger hospitals of the city came into being between 1850 and 1870.

From this time on three things are preeminently characteristic of the affairs of the Academy: periodic and ever more urgent complaints about the Academy's inadequate housing facilities; a vastly expanding program of scientific meetings, investigations, and reports, and an ever increasingly active concern with public health matters.

The library soon bulged out beyond the confines of its assigned stacks and rooms. In 1877 the library contained, not counting duplicates, 38,320 volumes. In the following year, the Governors of the New York Hospital donated some 23,000 volumes. With this addendum the library became the second largest medical library in the United States. Five years later the crowding became so acute that it was necessary to stack books on the floor, and readers were often obliged to read standing in the hallways.

The finances of the Academy were but little better than they had been in previous years. Yet this state of the treasury seemed to have had no ill effects on the Academy's scientific meetings. The programs at the turn

of the century list the names of men who since have been acknowledged as of the medical elect. Among those to be found on the roster of the 1901 Academy programs are J. B. Murphy, W. S. Halsted, W. C. Gorgas, William Osler, R. J. Fitz, and Abraham Jacobi. The subjects considered were timely and new; X-Ray Examinations; X-Ray in the Treatment of Cancer; Gonorrheal Infection; Backward Children and the Causes Thereof; Cirrhosis of the Liver; Human and Bovine Tuberculosis; The Widal Reaction; The Therapeutic Values of Ultra Violet Ray and of Radium. Reviewing the programs of those years one witnesses the unfolding of the vast panorama of modern medicine, with all of its thrilling achievements in the spheres of etiology, diagnosis, therapy, and prevention.

"There were giants on the earth in those days"—and the Academy was oft their host.

The modern public health movement grew apace with modern medicine, and so too did the Academy's interest in, and preoccupation with public health problems and matters. Tuberculosis was an outstanding concern of the Academy, as were the care and treatment of the insane, midwifery, public baths, abatement of the pollution of the atmosphere by smoke, cleanliness and purity of milk, food adulteration, and the fly and mosquito menace.

In 1905 Dr. C. L. Dana recommended to the Council that a permanent Committee on Public Health and Medical Economics be appointed. His recommendation was put into effect some six years later when a Committee on Public Health, Hospitals and the Budget was appointed by the President, William M. Polk. Dr. Dana was designated chairman of the Committee and its membership, numbering thirteen, included among others, Doctors A. G. Gerster, S. S. Goldwater, L. E. Holt, J. H. Hudleston, A. Jacobi, T. C. Janeway, J. A. Miller, and L. R. Williams. This Committee has since been renamed the Committee on Public Health Relations.

In 1905 the Academy also initiated the delivery of public lectures on health matters, and while this practice was soon abandoned, the delivery of the first lecture on December 29, 1905, by Dr. Thomas Darlington, Health Commissioner, was very favorably commended in the *Medical Record*. In light of the fact that the Academy did not establish its Medical Information Bureau until a quarter of a century later (1930), nor its Laity Lectures until 1935, the *Medical Record's* editorial comments in

1905 are particularly interesting and significant. "Hitherto," the editorial read, "the newspapers have been forced to rely for their information regarding medical topics of public interest upon interviews with individual physicians, a system liable to great abuses, and one exposing the complacent 'interviewees' to the often unjust accusation of 'advertising.' These public lectures under the auspices of the Academy of Medicine will do away in a measure with the newspaper need of individual interviews, but the principle might, we think, be extended with advantage. . . ." It also stated that if properly arranged such lectures would be to the laity what the Harvey Lectures were to the profession.

The outstanding preoccupation of the Academy's officers and Fellows was the inadequacy of the facilities available at the 43rd Street building. The Council room, heretofore reserved for the meetings of the Council, was yielded to the library. The platform of Hosack Hall was reduced to make room for eighty additional seats. Pamphlets were stored in the duplicate room. But all this was to little avail. The Academy had outgrown its quarters and there was nothing to do but to change them for larger ones. In 1909 a Committee on increased facilities was appointed. This Committee gave way to another, which in turn gave way to still another, in a tragi-comic series that ran for some fifteen years. In retrospect it appears that few of the *dramatis personae* fully appreciated the magnitude of the events transpiring. They wanted to amend where nothing but recasting to ample dimensions could suffice.

In the meantime the first World War intervened. When it was over, many men had acquired larger visions. The affairs of the Academy then moved ahead with amazing momentum.

Three among the Academy's Fellows paced the events which here must be told in extreme brevity. They were: Dr. D. Bryson Delavan, Dr. Arthur B. Duel, and Dr. Linsly R. Williams. In 1921, while the Council was still considering the *extension* of the old facilities at 43rd Street, Dr. Delavan, a member of the Council, composed and addressed a long communication to the President, George David Steward, in which he took exception to the Council's proposals on the ground that they envisaged no more than makeshift remedies. Dr. Delavan emphasized the fact that the Academy was an educational institution, and as such required endowment for its maintenance and continued growth. He proposed that the Academy should solicit the necessary endowment. His proposals were not received with enthusiasm; some considered them unpractical and a

dream. Nevertheless the Trustees were persuaded to appoint two Committees—one on Plan and Scope, with Dr. Delavan as chairman, and a second on Building and Finance, under the chairmanship of Dr. Duel. It was the latter who under fortuitous circumstances enlisted the interest and support of both the Carnegie and the Rockefeller Foundations, so that between them provisions were made for a new building and for some portion of the necessary administration budget. The grants from the Foundations, supplemented by contributions from Academy Fellows and their friends, and added to the monies derived from the sale of the old properties, made possible the present building with its extended services to the profession, the public, and the state. These possibilities, however, were realized only through the inspired labors of a host of men, among whom Dr. Linsly R. Williams was the most outstanding.

Linsly R. Williams, a Fellow of the Academy since 1904, became on January 1, 1924, its first Director. Into his capable hands passed the task of bringing the new Academy into existence. He labored with skill, tact, and vision, performing enormous tasks, and inspiring all who worked with him to do their utmost and their best.

The corner stone of the Academy's new building at 103rd Street and 5th Avenue was laid with appropriate ceremonies on October 30, 1925. By December of that year the building was one-third completed. The library, now counting in its principal division 133,000 volumes, was removed to the new quarters on August 2, 1926. The new building was opened officially on November 18, 1926, the event being celebrated with a dinner at the Waldorf on the preceding evening.

While the new building was being erected, and for some time previous, earnest consideration was given to the reorganization of the Academy. Principal among the accomplishments were the creation, during 1924, of the Bureau of Clinical Information, and the organization of the Committee on Medical Education.

Now the Academy entered upon a new era. While it had never been provincial, the Academy, because of its expanding field of interest, now gained a far-reaching influence. Its studies and pronouncements, its opinions and counsels, commanded respect. Many instances could be cited in witness; one must suffice: The studies and report of the Academy's Committee on Maternal Mortality aroused world-wide interest, and without doubt substantially contributed to the phenomenal reduction in maternal mortality rates that followed on their publication. Under

the skilful and progressive leadership of Dr. Linsly R. Williams the Academy became a preeminent institution, and a leader in the advancement of medical education, public health, social medicine, and public health education. The latter was of particular concern to Dr. Williams. His experiences as Deputy Health Commissioner, under Dr. Hermann M. Biggs, as Assistant to the Officer in Charge of Civil Affairs in Occupied Germany (following the first World War), as Director of the Rockefeller Commission for the Prevention of Tuberculosis in France, and as Managing Director of the National Tuberculosis Association, endowed him with a deep and ample appreciation of the fundamental importance of public health education. In that conviction he was subtly aggressive in his efforts to break down the profession's brahminical aloofness, for he saw in it a growing menace both to the medical profession and to the public.

He was not always successful in persuading his colleagues on these scores, but with patience, perseverance, and infinite tact, he was able to initiate and to establish new services which placed the Academy at the forefront among medical organizations in health education. It was during Dr. Williams' régime that the Academy organized its Committee on Medical Information (1928), and established its Medical Information Bureau (1930). In 1935 the Academy inaugurated its annual series of Laity Lectures, and in 1940 its Annual Health Education Conference.

It was much in the same spirit, of an earnest concern with the public relations of medicine, that the Academy organized in 1943 its Committee on Medicine and the Changing Order. The publication of this Committee's report, following upon four years of wide and intensive study on the ways in which more and better medical service may be provided to our people, happily concurs with the Academy's Centenary Celebration. It is an earnest attestation that those who followed the Founders have been faithful to the ideals upon which the Academy had been founded—to elevate the character of the profession, and to advance the science of medicine for the greater good of the people.

As to the future: It is not the function of the historian to play the role of prophet, and yet it is perhaps the most legitimate use to which history can be applied—to anticipate the future in the retrospect of the past.

The Academy came into being to subserve a need. That need has grown with time. As the practice of medicine becomes, as surely it will,

ever more intricately involved in economic and political determinants, so the more necessary will it become that singular attention be paid to *the science of medicine per se.* For otherwise medicine is likely to bog down in expediency and administrative routine. The safeguard against this lies in the exercise of a critical intelligence which is neither biased nor harassed by partisan interests and passions.

It is in these directions, and with regard to its past, that the Academy may look to its future; and in these connections, solicit and hope for the support of all who subscribe to the faith that Where there is love of Man, there too is love of the Art.[1]

[1] In the preparation of this text the author has drawn freely on Dr. Philip Van Ingen's forthcoming *History of The New York Academy of Medicine,* and herewith acknowledges his debt with gratitude.

# Virchow's Contribution to the Cell Theory

J. WALTER WILSON*

IN 1858 a young man of 37 gave a series of lectures in Berlin to a group of medical men. These lectures, later published under the title *Die Cellularpathologie,* ushered in a new epoch. The young man was Rudolf Virchow. He has been acclaimed, perhaps a little extravagantly, as the greatest figure in medicine since Hippocrates. By applying the cell theory of Schleiden and Schwann to pathology he transferred medicine from its ancient foundation on the four humors of the Greeks to a new foundation, the fundamental unit of modern biology, the cell, and transformed pathology itself into a modern science. At the same time, in the same book, he established the dictum *omnis cellula e cellula,* which has proven to be one of the most important axioms of biology.[1] Thus, in these lectures we find epitomized the interaction between medicine and biology which has repeatedly contributed to the advancement of both.

It is not the purpose of this paper to discuss in detail the rise of the new pathology. That is a task for the *medical* historian. But it is necessary to emphasize its significance. From the time of the ancient Greeks, the orthodox conception of disease and the theory of its treatment were based on the four humors which were related to the four elements and to the four qualities that the Greeks believed to be the ultimate components of the world—the elements, Fire, Water, Earth, and Air, and the qualities, Hot, Cold, Dry, and Wet. The four humors whose mixture determined the complexion of the body were Yellow Bile, Phlegm, Blood, and Black Bile. When these were properly mixed, the body was healthy; departure from the correct proportions produced disease. This unbalance, dyscrasia, could be corrected by treatment. The procedures of bleeding, cupping, and purging were based on this theory. So was the elaborate polypharmacy of Arabian medicine in which different drugs had a quality, or qualities, in varying degrees, but could be compounded in such a way as to counteract any type of unbalance of the humors. As Campbell

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[1] R. Virchow, *Die Cellularpathologie* (1858). Translated by Chance (New York n.d.).

has pointed out, this system was the most perfect, logically, that medicine has ever had, its only fault, from the viewpoint of logic, was that the *premises* were wrong. That an erroneous system should have dominated medicine for two millenia is difficult to explain, but the attractiveness of this logical perfection is undoubtedly part of the explanation.

When Schleiden and Schwann propounded the cell theory in 1838 the great pathologist of Vienna, Rokitansky, attempted to adapt the new theory to the ancient doctrine of humoral pathology, thus giving the old system a new lease of life. It was Virchow who put the old system to rest. His *cellular* pathology was the corollary of the cell theory, viz. that all functions of the body are cell functions. Muscle contraction is the contraction of muscle cells, and glandular secretion is secretory action of gland cells. Disease is a departure from normal function. Therefore, disease must also be sought in the cells. Many have pointed out that since Virchow's time the great discoveries in immunology, serology, and endocrinology have proven him wrong and restored the humors, in part at least, to an important place in medicine. This view seems to me to miss the main point of Virchow's contribution which cannot be contested, that is, that medicine should take as its basis not the fanciful systems of the Greek philosophers but the principles of modern science. Since Virchow's time there has been no return to the ancient humoral pathology. But much science of which Virchow could have had no knowledge has been incorporated into medicine.

Virchow's contribution to theoretical biology was also of great and lasting significance. It is one of the remarkable facts of the history of thought that within three years in the middle of the last century three men working in three very different fields and with no evident knowledge of, or interest in, the work of each other, published works in which the same general principle was established—the law of the continuity of life—a principle as important in biology as the law of the conservation of energy is in physics. In 1858, Virchow published in his *Cellular Pathology* the statement that all cells come from parent cells; in 1859, Darwin published his *Origin of Species,* which said in essence that species are not specially created but come from parent species; and in 1860, Pasteur published his researches that finally proved that no life, not even the most humble bacteria, could arise in non-living matter. Cells only from cells; species only from species; and organisms only from organisms. This means that life is a continuum handed on from cell to cell, from

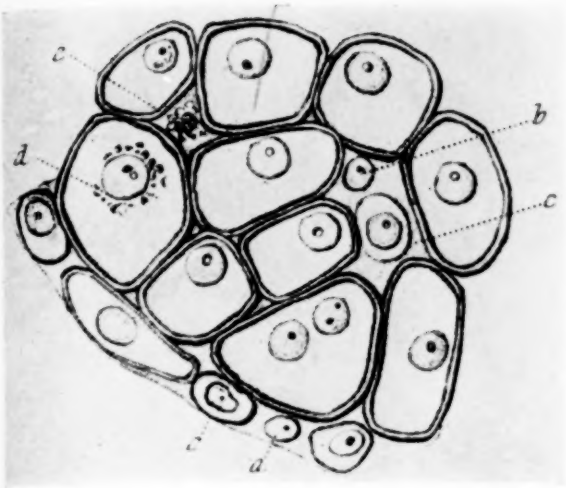


Fig. 1: After Schwann; Cartilage of the frog.

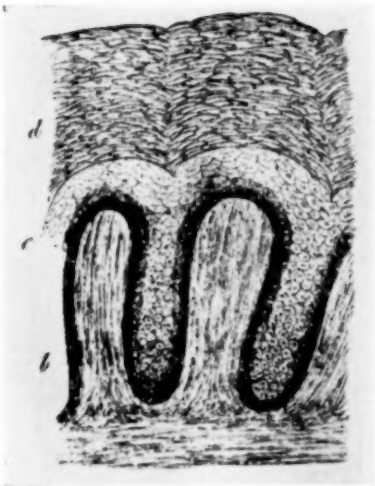


Fig. 2: After Kölliker. Perpendicular section of human skin.

Fig. 3: After Virchow. Bone, showing corpuscles and canaliculi.

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organism to organism, from species to species. It is the foundation stone of modern biology. My old chief, Dr. Mead, had a way of impressing it on students:

> Every time you wash your hands, dead cells go down the sink. The death of these cells was the first death that has occurred in a continuous chain of life extending back through the history of your body to the fertilized egg from which you arose, thus back through your parents, grandparents, and ancestors' bodies to the first human beings, and thus back through the ancestral non-human species of mammals, reptiles, amphibians, fish, and invertebrates from which man evolved, to the first spark of life that somehow in some primeval time originated on the surface of the earth.

It is the history of the cell theory and its contribution to medicine, and particularly Virchow's contribution, that I propose to discuss in this paper. That Virchow should have been interested in the cell theory was natural. He had just come to Berlin as a boy of eighteen in 1839, the year that Schwann published his *Microscopic Researches* and set the cell theory before the world. Under the leadership of the great teacher Johannes Müller, a brilliant group of men was assembled in Berlin, among them Schwann, Henle, Remak, and Reichert. The newly improved microscopes had been in use for only a few years and were constantly revealing new facts to the eager investigators. Years later Henle wrote "Those were the great times. Any day a bit of tissue shaved off with a scalpel or picked to pieces with a pair of needles, might lead to important and fundamental discoveries."[2] The importance of Schwann's theory had been immediately recognized as the element necessary for the organization of all these new facts. It must have been an exciting environment for a young man of 18 to find himself in.

In the past few years, particularly in the symposium to celebrate the centennial of the cell theory in 1938,[3] the reputation of the cell theory of Schleiden and Swann seems to have been progressively sinking, and we gain the impression that there was very little that was new in their theory and that that little was wrong.

In any attempt to appraise the historical significance of a scientific publication, there are two dangers, closely related. First, of reading into the speculations of antecedent works the ideas of the modern world, or at least to give the words and language of older writers connotations that such words and language have acquired as scientific theory has pro-

[2] Cited from Victor Robinson, *Pathfinders in Medicine* (New York 1929) 391.

[3] *Biological Symposia*, Vol. 1 (Lancaster, Pa. 1940).

gressed; and second, of expecting to find in the first statement of a theory the refinements that it accumulates as its foundations and implications are investigated by new generations of investigators with new techniques.

Professional historians have a great deal to say about the value of historical perspective in the appraisal of the significance of events. Unquestionably such perspective is of value in dealing with political and social history. It may even be necessary in the appraisal of scientific advance, but it must be applied with caution and with a thoroughgoing knowledge of contemporary events, especially of the new techniques that contributed the new facts on which the theory is founded, and also of the effect the theory had on the work of contemporaries. That the theory may contain elements that have been thought of and discussed before is no serious fault. New ideas grow out of old ones, not out of the vacuum of ignorance. The cell theory, for example, is a particulate theory of life. There have been particulate theories of life since the days of Anaxagoras and the Atomists of ancient Greece. It represents, apparently, one way that the human mind naturally works. Brock in editing the writings of Galen, discovers that some statements of Erasistratus "may be almost looked upon as an anticipation of the cell."[4] The authority of Aristotle with his *horror vacui* had overshadowed and suppressed this sort of thinking in the ancient and medieval world, but with the revolt against Aristotelian thinking that followed the Renaissance, it came again into prominence. The development of the microscope in the seventeenth century led to the hope that the ultimate living particles would soon be seen. In the middle of the seventeenth century, Buffon in his great *Histoire naturelle* formulated a particulate theory based on the "living molecules" which can be seen with a microscope:—for example, in the testis of a dog, or in any once living matter as it undergoes disintegration. With the crude microscopes of his day the fine differences between "infusoria" found in putrefying matter were not conspicuously visible. This theory was widely read throughout the cultured world and influenced non-scientists as well as scientists. It is not surprising to find in many important works of the early nineteenth century ideas that foreshadowed those of Schleiden and Schwann. Oken, the great German nature-philosopher claimed priority[5] over Schleiden and Schwann on the basis of speculations published in 1805, and there can be no doubt about

[4] Galen, *On the Natural Faculties.* Translated by A. J. Brock (New York 1916) 153.

[5] L. Oken, *Elements of Physiophilosophy.* Translated by Tulk (London 1847) xi.

his influence on their thinking. A good case has been made by Dr. Rich for Dutrochet not only as a predecessor of Schleiden and Schwann but as the real father of the cell theory.[6] In 1824, Dutrochet had published his researches on the ultimate structure of animals and plants and had talked of globules seen everywhere in plants and animals under the microscope. In some places his language was so nearly like that used later by Schwann that Dr. Rich was led to accuse Schwann of plagiarism. It seems to me important that Dutrochet lived for a decade after Schwann's book was published, during which it was receiving universal acclaim, and never himself claimed priority. The fact is that though the language was similar the ideas themselves were different. If one is discussing any particulate theory of life, there is bound to be a similarity of words and phrases; that is why language is understandable.

There was real novelty in Schwann's ideas and the clue to their novelty is to be found in the facts he used and the techniques by which these facts were discovered. Real progress in science is based on new facts, and new facts are the product of new techniques.

The improvements in the microscope between 1826 and 1830 were responsible for the new facts that led to the cell theory of Schleiden and Schwann. The most important improvement was the achromatic objective invented by Amici in 1826 and immediately improved by Lister and others. The importance of this can be realized by anyone who will take the trouble to compare the performance of a toy compound microscope with one of our students' instruments of today. After the obvious discoveries when the microscope was first invented in the seventeenth century, the use of the microscope had declined in science because it was so unsatisfactory. It was little more than a plaything of amateurs. In spite of the fact that it was constantly improved, Bichat, the father of histology, not only did not use it but was distrustful of observations others made with it. The globules that Dutrochet discussed were optical artefacts produced by poor lenses and very bad illumination. His pictures show this to be true. Furthermore, we have the testimony of such men as Henle and Virchow, and of contemporary books on the use of the microscope. Virchow says, "this view [of omnipresent globules] was in part attributable to optical illusions in microscopical observation. The objectionable method which prevailed during the whole of the last and part

[6] A. R. Rich. "The Place of R. J. H. Dutrochet in the Development of the Cell Theory," *Bull. Johns Hopkins Hosp.* 39 (1926) 330.

of the present century—of making observations (with but indifferent instruments) in the full glare of the sun—caused a certain amount of dispersion of light in nearly all microscopical objects, and the impression communicated to the observer was, that he saw nothing else than globules."[7] Dutrochet considered these globules to be the elementary particles of animal and plant tissues. The idea of the elementary living particle persisted long after Schleiden and Schwann. It is still extant in the mitochondrion and the gene, and other self-perpetuating units that some consider, rather than the cell itself, to be the ultimate units of life. Detrochet's theory was thus, in a sense, the predecessor of a rival particulate theory rather than of the cell theory itself.[8]

The ultimate unit of Schleiden and Schwann was one that could not have been conceived before the new microscope with achromatic lenses came into use. It was the nucleated cell. The nucleus had been discovered in plant cells in 1831 by Robert Brown and had been described in many cells both animal and plant before Schleiden and Schwann seized upon it as the general feature of their unit of organization. The new microscope created a furore of work. As Henle said, it was only necessary to scrape the surface of some organ with a scalpel and examine the scrapings under the microscope to make a discovery. Huxley said in 1853:

> At a time when the researches of innumerable guideless investigators, called into existence by the tempting facilities offered by the improvement of microscopes, threatened to swamp science in minutiae, and to render the noble calling of the physiologist identical with that of the "putter-up" of preparations, they [Schleiden and Schwann] stepped forward with the cell theory as a colligation of the facts. To the investigator, they afforded a clear basis and starting point for his inquiries; for the student, they grouped together immense masses of details in a clear and perspicuous manner. Let us not be ungrateful for what they wrought. If not absolutely true, it was the truest thing that had been done in biology for half a century.[9]

There were two aspects of their concept of the cell theory that were wrong—the nature and importance of the cell membrane, and the method of origin of cells. It is hard to see how they could have been right, however, for it took a host of workers, largely inspired by their cell theory, twenty years to arrive at our modern idea of protoplasm, and nearly forty years to work out the complete story of cell division. He must be complacent indeed who would scoff at the thread of error that runs

[7] Virchow, *op. cit.* 52.

[8] J. W. Wilson, "Dutrochet and the Cell Theory," *Isis* 37 (1947) Pt. 1-2.

[9] T. H. Huxley, "The Cell Theory," *Brit. and For. Med.-Chir. Rev.* 12 (1853) 285. Reprinted in *Scientific Memoirs* (London 1898) 1. 242.

through the fabric of scientific thought! Who would contend that we today possess ultimate truth? It seems superfluous for a writer today to emphasize the error of Schwann's theory, when Schwann himself had the following to say of it.

> An hypothesis is never hurtful, so long as one bears in mind the amount of its probability, and the grounds upon which it is formed. It is not only advantageous, but necessary to science, that when a certain cycle of phenomena have been ascertained by observation, some provisional explanation should be devised as closely as possible in accordance with them; even though there be a risk of upsetting this by further investigation; for it is only in this way that one can rationally be led to new discoveries, which may either confirm or refute it.[10]

The error about the cell membrane was a natural one. From the earliest days of the microscope the cellular structure of plant tissues had been known. Robert Hooke had used the word *cell* in 1665, in connection with the cell walls in cork, a dead plant substance. In the works of Haller, the essential substance of the animal body was cellular tissue; what we would today call areolar tissue or simply connective tissue. The characteristic material of each organ (what we would call the parenchyma) was merely accumulated in the compartments of the cellular tissue, incidentally.[11] This concept was adopted and elaborately discussed by Bichat. Schwann's theory elevated the parenchymal substance to its present importance, but still retained the idea that it was made up of cells whose membranes were the important features of their structure. We have transferred our idea of the living substance to the whole protoplasm, but even today in discussions of osmotic phenomena for example, the membrane has for us the same significance that it had for Schwann.

He believed as we do that growth is essentially production of new cells, but his theory of the origin of new cells seems bizarre to us, for it was a theory of spontaneous generation. The cells were considered to arise by a process that he compared to crystallization, out of a fluid matrix which he called *cytoblastema.* This is the interstitial fluid. It differs in different parts and derives its peculiar materials from the blood. His idea of the origin of cells may be illustrated by the account he gives of the cartilage of the frog.

> Plate III, fig. 1, [Fig. 1] e, appears to be a cell-nucleus of a cartilage-cell in the act of forming. A small round corpuscle is there seen, surrounded by some

[10] T. Schwann, *Mikroskopische Untersuchungen über die Uebereinstimmung in der Struktur und dem Wachsthum der Thiere und Pflanzen* (Berlin 1839). Quoted from translation by Smith (London 1847).

[11] J. W. Wilson, "Cellular Tissue and the Dawn of the Cell Theory," *Isis* 35 (1944) 168.

minutely granulous substance, whilst the rest of the cytoblastema is homogeneous. This granulous substance is gradually lost around the object; at a subsequent period it begins to be sharply defined, and then exhibits the form of a cell-nucleus, which continues to grow for a certain period. (See pl. III, fig. 1, a.b.)

The formative process of the nucleus may, accordingly, be conceived to be as follows: A nucleolus is first formed; around this a stratum of substance is deposited, which is usually minutely granulous, but not as yet sharply defined on the outside. As new molecules are constantly being deposited in this stratum between those already present, and as this takes place within a precise distance of the nucleolus only, the stratum becomes defined externally, and a cell-nucleus having a more or less sharp contour is formed. The nucleus grows by a continuous deposition of new molecules between those already existing, that is, by intussusception.

When the nucleus has reached a certain stage of development, the cell is formed around it. The following appears to be the process by which this takes place. A stratum of substance, which differs from the cytoblastema, is deposited upon the exterior of the nucleus. (See pl. III, fig. 1, d.) In the first instance this stratum is not sharply defined externally, but becomes so in consequence of the progressive deposition of new molecules. The stratum is more or less thick, sometimes homogeneous, sometimes granulous; the latter is most frequently the case in the thick strata which occur in the formation of the majority of animal cells. We cannot, at this period distinguish a cell-cavity and cell-wall. The deposition of new molecules between those already existing proceeds, however, and is so effected that when the stratum is thin, the entire layer—and when it is thick, only the external portion—becomes gradually consolidated into a membrane.[12]

If one examines the series of lymphocytes it is possible to make a similar story, and the stratified epithelium of the epidermis (Fig. 2), where the cells are arranged according to their generation, gives a particularly convincing illustration. The cytoblastema in this case is the fluid of the underlying tissue. It was the possible relation of this fluid cytoblastema to the humors of the body that led Rokitansky to attempt to adapt the new cell theory to the old humoral pathology.

The error in the importance of the cell *membrane* persisted for a long time, but the error about the *origin* of cells was immediately questioned, although "free cell formation" was still given as a possible method of production of new cells in Strasburger's monograph in 1875.

The first evidence of cell division came from plants where the cells are fixed in position, and from the cleavage of the animal egg where the process is obvious. Remak studied the process in embryology generally and became convinced that as a rule, cells are produced by division of parent cells. Kölliker, in his manual of histology published in 1853, discusses this as the *endogenous* origin of cells. He says: "The commonest form of this cell genesis is, that a so-called parent cell produces two sec-

[12] Schwann, *op cit.* 175.

ondary cells which from the first wholly fill it. First the nucleoli divide, then the nuclei, then a wall of separation arises between the cells which divides the parent cell into two perfectly distinct spaces."[13] It is important to emphasize that even in discussing the division of the cell, he was still thinking of it as a hollow vesicle. "The mode in which the multiplication of the nucleus takes place has not yet been made out with exactness." It was twenty years before the truth was known. He also describes, however, free cell formation—of lymph corpuscles, in Malpighian corpuscles of the spleen for example.

It was Virchow who finally formulated the generalization that *all* cells are formed from parent cells. He approached the question from pathology. One of his first published papers was an attack on Rokitansky's humoral pathology. The attack was so successful as to demolish it.

The idea of the origin of cells from parent cells was not new. Many students of embryology, among others, Remak and Kölliker, had observed cleavage of the egg, and as early as 1840 Reichert had pointed out that there is nothing to correspond to the cytoblastema of Schwann's theory in the early embryo. Remak put the concept of the three primary germ layers and their differentiation into the modern form, demonstrating that the ectoderm and endoderm produce epithelial structures in general—glands, for example, being produced by a proliferation into the underlying tissue of the superficial epithelia. Remak had even observed the division of blood cells in the chick embryo. Nevertheless, the idea of origin of cells *de novo* from tissue fluids still persisted. The origin of cells in inflammation was an example. The nature of cancer, entirely mysterious to ancient writers, began to be clarified, as soon as the cell theory was formulated, in a paper by Johannes Müller. He recognized that new cell formation was involved. The further elaboration of this concept was one of Virchow's contributions. It was his idea that the cells, both in inflammation and in neoplasms, come from preceding cells. He thus arrived at his generalization that *all* cells come from preceding cells. The idea of spontaneous generation was already disproved for most organisms and Virchow recognized that cell origin from a cytoblastema would be a violation of the general rule. He had himself written extensively against the idea of the spontaneous generation of parasitic worms.

[13] A. Kölliker, *Manual of Human Histology.* Translated by Busk and Huxley (London 1853) 21.

Just as little as we can now admit that a taenia can arise out of intestinal mucus, or that out of the residue of the decomposition of animal or vegetable matter an infusorial animalcule, a fungus, or an alga, can be formed, equally little are we disposed to concede either in physiological or pathological histology, that a new cell can build itself up out of any non-cellular substance.[14]

It was partly this theoretical consideration rather than the facts that led him to his generalization. He considered his idea so different from Schleiden and Schwann's cell theory that he thought of it as a new cell theory and in later life is quoted as having said it was his most important and enduring contribution.

Important and enduring as it was, it still involved two errors, almost the same as those of Schwann. First, a mistaken idea of the source of new cells in growth, and second, a mistaken idea of the nature and importance of the cell wall.

I will discuss the second error first, for it was soonest and simplest corrected by the development of the concept of protoplasm. In his general discussion of the cell this misapprehension is not obtrusive. But in his discussion of the importance of cells in the distribution of the nutritive juices, it becomes apparent that he considers the cell to be essentially a hollow compartment. Many large areas of the body are without capillary blood vessels and yet they contain many cells. This is particularly true of some of the connective tissues. Bone for example, is traversed by large vessels in the Haversian canals but has no capillaries (Fig. 3). The matrix is, however, filled with lucunae containing bone corpuscles and connected by canaliculi. These are the channels through which the nutrient materials reach the substance of the bone. The umbilical cord is another example. Virchow says:

The proper substance of the umbilical cord consists of a reticulated tissue, the meshes of which contain mucus (Mucin) and a few roundish cells, whilst its trabeculae are composed of a striated fibrous substance. In this lie stellate corpuscles, and when a good preparation has been obtained by treatment with acetic acid, a symmetrical network of cells is brought to view, which splits up the mass into such irregular divisions, that by means of the anastomosis which subsists between these cells throughout the whole of the umbilical cord, a uniform distribution of the nutritive juices throughout the whole of its substance is in this instance also rendered possible.[15]

In other words, hollow stellate cells anastomose to form continuous channels through which nutritive material is conveyed.

This concept of the cells and their relation to the tissues is obviously

[14] Virchow. *op. cit.* (see note 1) 54. [15] *Ibid.* 130.

very different from ours. It is an adaptation to the cell theory of an old concept of vessels smaller than capillaries, which were considered important in tissue nutrition. They had been considered to exist in the mucosa of the gut to take in the digested food, and in the glands to permit the products of secretion to escape from the blood. The concept is well developed in Bichat's writings. To him they were entirely hypothetical and invisible. These of Virchow were visible—the channels produced by anastomosing hollow processes of hollow cells.

Purkinje had invented the term protoplasm to apply to the substance of which the young embryo is made. Dujardin had recognized the jelly-like stuff of the ameboid protozoa as *sarcode*. And von Mohl had discovered the nitrogen-containing lining inside the cellulose wall of the plant cell. But the general idea that the cell is a mass of protoplasm rather than a hollow vesicle arose gradually from the study of living cells, particularly those that show movement—plant cells and their streaming, protozoa, and finally muscle cells. The idea of a living moving stuff is found in the work of Cohn in 1852. It gathered force gradually and in Leydig's *Histology* of 1857 the importance of the soft substance rather than the wall in the concept of the cell is already emphasized. The structure of muscle fibers had long been a puzzle. Sir Everard Home had described them as capillaries and considered the striations to be blood corpuscles packed in them. Virchow did not subscribe to this view in spite of the fact that the sarcolemma with the nuclei scattered throughout is very like a vessel. But he did consider the contractile substance as something extraneous contained in the cell, rather than the living stuff itself. That is why he called it "contents" as he did pigment and other material visible in cells. The fundamental element was for him the cell itself stripped of the contents.

It was Max Schultze who in 1861 in a paper "On muscle fibers and what one has called *a cell*" finally defined the cell in the terms we use today—as a mass of protoplasm with a nucleus. He concluded that the contractile substance of muscle is the same as the streaming material of plant cells and protozoa and that this stuff, protoplasm, is the real living material of the cell. With this, the modern concept of the cell was complete. It is true that we still discuss the membrane in physiology, and Dr. Just recently wrote an entire book on the *Biology of the Cell Surface,* but it is the whole substance of the cell, the mass of protoplasm, that we consider alive.

Virchow's concept of the origin of new cells was superficially like ours. He recognized that cells divide into two. He also believed, however, that some cells might act as brood cells and produce many. For a long time this was thought to be true, for example, of the cysts that produce spermatozoa, especially in invertebrates. This notion was corrected only when the division of the nucleus came to be understood. He denied that the cells could arise *de novo* from tissue fluids. The factors that contributed to his main idea that cells come only from cells were his own investigations on the structure of connective tissue, the observations of others, e.g. Remak and Kölliker, on the division of cells, and finally his distaste for the idea of spontaneous generation, even of cells. These were enough to give him the modern idea of cell genesis in general, but not the correct idea of cell generation in particular cases. The clue to his errors is to be found in his ideas of connective tissue and in his concept of metaplasia.

It had been believed that the cells of epidermis were produced from cytoblastema, which was the fluid in the connective tissue beneath it. Virchow proved by his own researches that even adult connective tissue contains cells, whereas previous investigators had believed that, if there were cells at all in young connective tissue, they disappeared with the formation of the fibers to which they contributed. Virchow believed that these cells pushing out, rather than cytoblastema, produced the epithelia —or, in exudates, pus cells and the cells of pathological new growths. The connective tissue thus became again what it had been in the writings of the early nineteenth century, a germinative tissue. The transformation its cells underwent to produce various structures he called *metaplasia.* We still use the term, but in an extremely restricted sense compared to his use of it. A few years later Cohnheim, after his discovery of the escape of white blood cells from the blood vessels by diapedesis, conceived that these cells rather than the connective tissue cells were the generative cells of the body. Many papers on the histology of regeneration in the 1890's ascribe the origin of new tissues to these migrating white blood cells.

It was the discovery of the facts of cell division, the demonstration of the mitotic process by the use of stains, that corrected this error.

The techniques necessary to demonstrate the details of mitosis were developed rapidly in the 1860's. They were the techniques of fixation, sectioning, and staining. There were also developed better objectives, and, still more important, the substage condenser which gave the micro-

scope, optically, at least, its modern perfection. The use of preservatives for tissues is very old, but the excellent fixatives we use today were invented only as they were needed to meet the demands of other refinements in technique. Purkinje had in the 1830's introduced the use of acetic acid to make the nucleus more conspicuous. But the various practices for getting the tissues thin enough to get light through under the microscope gave pretty crude preparations: scraping; slicing with the knife of Valentin; pressing in the compressorium. The methods of imbedding and the microtome gave incomparably thinner sections and required refined methods of fixation. All these came in the 1860's and early 1870's.

The general use of stains in histology came about as the result of an accident which happened the same year that Virchow delivered his lectures, i.e. in 1858. The injection of blood vessels with colored masses had been an important method of study with anatomists since the early eighteenth century. Gerlach had observed that sometimes after injection with some carmine masses the tissues became colored by diffusion of the dye from the vessels and that nuclei were particularly stained. He set out to devise a simple and certain method of staining. After considerable fruitless experiment he hit on a practical method by accident. He left a section of cerebellum that had been hardened in potassium bichromate in a very dilute solution of ammoniacal carmine over night by mistake, and found the fibers and cells beautifully stained. The method was very quickly taken up and modified by many workers. Other dyes, hematoxylin and the aniline dyes which were just being invented, came rapidly into use.

By the early 1870's there were in use many fixing agents—chromic acid, osmic acid, Müller's solution, and the like, and many dyes. It was at this time that Paul Mayer at the Naples Station began his lifelong work of systematizing the elaborate procedures that the histologist and the cytologist developed, a most necessary step in making these complicated techniques generally available to biologists. A flood of observations followed and the foundations of modern cytology were rapidly laid. The work of Auerbach, Van Beneden, Flemming, Fol, Hertwig, Strasburger, and many others in the 1870's gave us the modern description of the behavior of the chromosomes in mitosis. The beautiful monographs of Strasburger and Flemming were the climax of this development. Kölliker and others with him who had seen cells divide had asked what

happened to the nucleus when it seemed to disappear just before division and new ones reappear afterward. This was the answer. With this simple method of observing cell division—the demonstration of mitotic figures in thin sections fixed and stained, the true story of cell genesis in tissues became known. Waldeyer, for example, described the cell divisions in epithelia, and no longer were cytoblastema or cells from a germinative tissue beneath, required to account for its continued growth. This completed our modern conception of the cell and its genesis—the cell theory of today—which started its growth with the facts observed with the new microscopes in the 1830's.

To us, the cell is a mass of protoplasm with a nucleus. Its normal method of origin is by division of a parent cell by the process of mitosis, which is a very elaborate process, insuring the precise duplication of a complicated nuclear structure, a far cry from the vesicular cell and the cytoblastema of Schleiden and Schwann.

The further development of our knowledge of the maneuvers of the chromosomes in mitosis, in the maturation divisions of the germ cells, and in fertilization is another story. It must be mentioned here only because it leads through the work of Hertwig, Roux, DeVries, and Weismann to a new theory of heredity and of cellular differentiation. This had as a corollary the doctrine of tissue specificity—that is, that as the various tissues are developed in the embryo they assume a specific character which is fixed. A neuroblast can only produce nerve cells, a myoblast only muscle cells, and so on for each category of cells. This was, of course, contrary to Virchow's doctrine of metaplasia. In his lectures he had described all sorts of tissues, normal and pathological, arising by transformation of the cells of connective tissue, which he considered, as we have seen, a true germinative or formative tissue. He had also described the origin of pus cells from the epithelial cells of the surfaces on which exudates appear. To him metaplasia occurred as a very general phenomenon. The new knowledge quickly corrected the most obvious errors of his theories, but even today the vestiges of his ideas are to be found, perhaps with justification, in the literature about the behavior of the connective tissue and blood-forming tissue in pathological processes.

Virchow made one other important contribution to the cell theory, and that is the development of the analogy between the organism as a whole and the state. Just as the state is made up of many individuals with different functions, so is the organism made up of cells with different

functions. There is no single seat of life in the organism. Its life is found dispersed in all the living units. The unity of its life is nothing more than the interaction of the living units. In his own words:

> Every animal presents itself as a sum of vital unities, every one of which manifests all the characteristics of life. The characteristics and unity of life cannot be limited to any one particular spot in a highly developed organism (for example, to the brain of man) but are to be found only in the definite, constantly recurring structure, which every individual element displays. Hence, it follows that the structural composition of a body of considerable size, a so-called individual, always represents a kind of social arrangement of parts, an arrangement of a social kind, in which a number of individual existences are mutually dependent, but in such a way, that every element has its own special action, and, even though it derives its stimulus to activity from other parts, yet alone effects the actual performance of its duties.[16]

The idea was seized upon by Haeckel and others and built into the framework of orthodox biological thought. It has not, however, gone unchallenged, for it has been difficult for many investigators to believe that the exquisite integrity of the individual organism that is expressed in the coordination of processes in physiology and in development could possibly be obtained through the mere interaction of a host of separate individuals. They have sought either a more substantial unity in the various syncytial theories or a more esoteric unity in the theories of vitalism. But throughout, the cell state concept has persisted as the middle-of-the-way course, and has satisfactorily answered most of the questions of all but the most persistent inquirers.

By way of conclusion I would like to draw a moral from the history we have been discussing. It is not original with me but needs repeated reiteration for those who deal with the history of thought. Great theories do not spring Minerva-like from the heads of great men and their full import cannot be encompassed in a brief dictum.

The cell theory of today could be simply stated as follows: The living organism is composed only of cells and products of cells. These cells are the ultimate living units. Each is a mass of protoplasm with a nucleus. They arise by a division of preceding cells by mitosis. The unity of the organism derives only from interaction of the cells, not from their fusion into a single syncytial mass nor from the action of some vitalistic principle that transcends them.

This is not the cell theory of Schleiden and Schwann; nor of Virchow, nor of Max Schultze; but it is the product of all of them and many more, refined in the crucible of ever new experience.

[16] *Ibid.* 40.

In discussing the history of thought it is futile to attempt to belittle the work of one man by pointing out how he has been anticipated by others, or what aspects of his work have later been proved wrong. It seems equally futile to say that in view of the fact that all discoveries are based upon previous ones and that science is possible only by such co-operation we might as well leave the personalities out. It is true, all discovery and all science are, indeed, social functions; but it is equally true that there is nothing in society that transcends the individuals of which it is composed. The advance of science is the record of the achievement of men. The honor we pay them is their greatest reward.

# Sir William Osler's Literary Investigations

WILLIAM WHITE*

HIDDEN away in the somewhat obscure *Medical Library and Historical Journal* is an essay read by Sir William Osler in January 1900 before the Johns Hopkins Hospital Historical Club on "The *Phthisiologia* of Richard Morton, M.D." The paper begins:

> August 22, 1662—Black Bartholomew's Day, as it has been called—brought sorrow and sadness to many English homes. The enforcement of the Act of Uniformity called for subscription to the Thirty-nine Articles, and enforced the use by all clergymen of the Book of Common Prayer. Among those ejected for refusal to subscribe—2,000 in number, it is said—was a young man, aged twenty-five, the Vicar of Kinver, in Staffordshire, Richard Morton by name.[1]

Osler proceeded to give an account of Morton's life, his connection with medicine, and in quite some detail a review of Morton's most important work, the *Phthisiologia,* 1689. This book is chiefly of value today as one of the first treatises on pulmonary consumption, and Morton's work antedates by 200 years Cohnheim's and Virchow's dictum on the prevalence of tuberculosis of the lungs.

This essay by Osler, while it is of greater value to medicine than to literature and is another instance of his unerring eye for historical priorities, has been chosen as the beginning of the present study for two reasons: it is Osler at his literary or dramatic best, opening a paper with a colorful picture (as in his fine essay on Servetus); and the essay was the discussion of a book, a work of literature, although to be sure, medical literature. Naturally, as a physician, his first concern was with medical literature; but it was certainly not his exclusive concern.

Again and again he delved into literature—and history, too—almost for their own sake. Two of his best known investigations which seem primarily literary are the *Bibliotheca Osleriana,* which was actually completed and published after his death, and the pamphlet *Creators, Transmuters, and Transmitters as Illustrated by Shakespeare, Bacon, and Burton.* Both of these I have previously discussed, the *Bibliotheca* from the standpoint of the novels mentioned in it,[2] and the latter at much greater length as an Introduction to a reprint of *Creators, Transmuters, and*

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[1] Sir William Osler, "The *Phthisiologia* of Richard Morton," *Med. Lib. and Hist. Jour.* 2 (January 1904) 1.

[2] William White, "Sir William Osler as a

*Transmitters.*[3] I should like now to take up the *Bibliotheca Osleriana* as a whole, and the separate papers of Sir William Osler, such as "Physic and Physicians as Depicted in Plato," "The Earliest Printed Medical Books," "Books and Men," "The Anatomie universelle of Ambroise Paré" and "The First Printed Documents Relating to Modern Surgical Anaesthesia." Then there were a few less important items like "A Review of F. P. Weber's *Aspects of Death and Correlated Aspects of Life in Art, Epigram and Poetry,*" "A Note on Currie's Journal," "Some Aspects of American Medical Bibliography," and his "Address to the International Association of Antiquarian Booksellers."

## I

Osler's fame as a great clinical teacher was aptly balanced by his literary activities which brought him repute as a bibliographer. This work, far more than the mere "recreation" listed in *Who's Who?,* considerably heightened and broadened his powers as an inspiring teacher and student of medicine. "No small part of Osler's broadly liberal culture came from studies in medical history, biography and bibliography, which almost inevitably led far afield into the realms of classical and general literature and of social, political and philosophical history,"[4] writes W. H. Welch.

Those who do not know the man will be amazed that bibliographical and other literary investigations led him so far from the medical fold, but his works in the science of literature were prepared with the same care, the same depth of research, the same broad background as were his many contributions to the literature of science. Behind these works stood many years of intense interest in books and bibliography, in the pursuit of which he built up an exceptionally fine library. This library he decided to catalogue according to his convictions of what a catalogue should be (really a critical compendium), and accordingly the work was begun, shortly to be interrupted by World War I.

*Bibliotheca Osleriana,* the great catalogue compiled by Osler and partly by the editors whom he chose before his death, is an enormous summary, so to speak, of all his work of a bibliographical nature. Pub-

Critic of the Novel," *Med. Rec.* 148 (November 2, 1938) 340-42. Cf. also "The Literary Physician: A Note on Osler's Essays," *California and Western Medicine* 54 (February 1941) 79-80.

[3] William White, "Osler on Shakespeare, Bacon, and Burton, with a Reprint of his *Creators, Transmuters and Transmitters as Illustrated by Shakespeare, Bacon, and Burton,*" *Bull. Hist. Med.* 7 (April 1939) 392-408.

[4] Foreword, *Bulletin of the International Association of Medical Museums* (Montreal, 1927) vii.

lished by the Clarendon Press, Oxford, ten years after its author's death, this Catalogue of Books, arranged and annotated by Sir William Osler and bequeathed to McGill University, was dedicated to the memory of Dame Grace Revere Osler; it contains, in addition to the catalogue, Osler's introductory essay, "The Collecting of a Library," and two shorter notes, "A Record Day at Sotheby's" and "Distribution of Special Books to Other Libraries."

"The Collecting of a Library" is largely the autobiographical account of the growth of Osler's own library, from the Globe Shakespeare and the 1862 edition of the *Religio Medici* to the collection he eventually gave to McGill. He pays glowing tribute to the three most influential men in his life: the Reverend W. A. Johnson, Dr. James Bovell, and Dr. Palmer Howard. And then, in an account of his early years, he tells the oft-repeated tale of the copy of Schmidt's *Shakespeare-Lexicon* he ordered from Berlin in 1873 for his Montreal Library. When the concordance finally came in October 1875, Osler had taken rooms with Mr. King, an Englishman employed in the Custom House who had but one thought in life: Shakespeare. The old man, in whose library "WO" had passed much time, would be extremely happy to see the lexicon:

> I looked at it hurriedly but with much anticipatory pleasure. On my return to the house Mr. King, who had just come in, was sitting by the fire and greeted me in his cheery way with, "What's that you've got?" "Something that will rejoice your heart," I said, and deposited the work in his lap. The shock of the realization of a life-long dream, a complete concordance of Shakespeare, seemed to daze the old man. He had no further interest in me and not a word did he say. I never got it back! For months he avoided me, but helping him one day on the stairs, my manner showed that Schmidt was forgotten, and he never referred to it again. The work went to McGill College with his Shakespeare collection. When in the Library in 1910, I asked for the first edition of Schmidt and was glad to see my book again after thirty-five years. This story is written on the flyleaf [cf. no. 5451] as a warning to bibliomaniacs![5]

Osler then goes on to tell of his interest in libraries during the years he spent in Philadelphia and Baltimore. In the former city he became associated with the library of the College of Physicians and improved, he said, his general education. In Baltimore, with a larger income, he had begun "to buy, first, the early books and pamphlets relating to the profession in America; secondly, the original editions of great writers in science and in medicine; and thirdly, the works of such general authors

[5] Sir William Osler, *Bibliotheca Osleriana* (Oxford 1929) xix. Cf. also Harvey Cushing, *The Life of Sir William Osler* (Oxford 1925) I. 138.

as Sir Thos. Browne, Milton, Shelley, Keats and others."[6] The years in the United States brought, he wrote, "troops of friends whose affection became part of my life," and he came in sympathetic touch with those friends of the spirit, the great and good men of the past."[7]

With the growth of the collection at Oxford, he began to make plans for its ultimate disposition. To the memorial which Sir William and Lady Osler decided to dedicate to their son, Revere, went the works in general literature, together with Dr. Osler's original editions of Milton, Fuller, Donne, Shelley, and Keats. Medical and scientific books were to go to McGill. "Gradually, as the books increased, the hope matured into a scheme for a library which would have (a) a definite educational value, (b) a literary, and (c) an historical interest;" and this is his divisional plan:

I. Prima, which gives in chronological order a bio-bibliographical account of the evolution of science, including medicine.

II. Secunda, the works of men who have made notable contributions, or whose works have some special interest, but scarcely up to the mark of those in Prima.

III. Litteraria, the literary works written by medical men, and books dealing in a general way with doctors and the profession.

IV. Historica, with the stories of institutions, etc.

V. Biographica.

VI. Bibliographica.

VII. Incunabula, and

VIII. Manuscripts.[8]

There then arose the desire to print a *catalogue raisonné,* and he planned to write introductions to each section (which, unfortunately, were never written). Because of the heavy labor this involved, he asked that if he should die before it was completed, Lady Osler, with L. L. Mackall, W. W. Francis, and Archibald Malloch should finish it. He expressed a hope that he would be able to make provision for its extension and upkeep. "The books," he wrote, "have come from three sources: sales, catalogues and second-hand book shops."[9]

Here end Osler's remarks toward the text of the contemplated general Introduction. Notes for its continuation, such as the record day at Sotheby's and a short account of an auction at Hotel Drouot, Paris, are included in the Introduction as now printed; other notes, such as an intended introduction to Bibliotheca Litteraria, have been incorporated into

[6] Osler, *Bibliotheca Osleriana* xxii.
[7] *Ibid.* xxiii-xxiv.
[8] *Ibid.* xxv-xxvi.
[9] *Ibid.* xxvi.

the catalogue. In their own Preface, Editors Francis, Malloch, and R. H. Hill (of the Bodleian Library) explain in greater detail the divisions of the library, particularly Prima and Litteraria. The former section includes bibliographical data on the works of 67 contributors of the first rank to the advancement of science, beginning with Hippocrates and ending with Roentgen. It is surprising to find included in this section Conrad Gesner, of whom Osler said, "I am not sure that this fellow should go into 'Prima'; but I love him so much that I must put him there. Besides he is the father of Bibliography."[10]

On another point, cross-references, Osler ruled that they should be reduced to a mimimum, except in the index. When friends protested that it would be impossible to decide into which section certain books should be put—science, literature, or history, when they might belong to all or none—he said, "Well, print in large letters at the top of each page, CONSULT THE INDEX FIRST."[11]

In addition to what he had to say in the *Biblotheca Osleriana,* in the Introduction and elsewhere, Osler wrote again and again to his legion of correspondents about his growing collection. Cushing's *Life* refers to the *Bibliotheca* more than one hundred times, mostly citing letters during the last years of Sir William's life, such as these:

> Library thrives—catalogue finished and all shelves marked—a 1673 apocryphal ed. of Le Malade Imaginaire came in yesterday—I find no reference to one of that date.
>
> I am struggling with a proper catalogue of my library. Keep an eye open, please.
>
> Two treasures recently for Bibl. prima—Copernicus De Revol. Orb. celes. 1543, & the Ed. Prin. of Plato—Aldine 1513. P. comes as the founder of psychology.
>
> It will take five years at least to furnish the notes, so that it's a race between the Catalogue and Pallida Mors.[12]

As early as 1911 the scheme of classification was decided upon, for a deed of gift drafted that year mentions the eight sections into which the library was later divided. By the time of Osler's death in 1919 the outline for the catalogue was sketched and the form and general contents of Bibliotheca Prima settled. Catalogue cards had been arranged in one alphabetical series and were being rearranged, but the exact place of many of the works was still undecided. Miss J. F. Willcock, an experi-

[10] *Ibid.* No. 623.
[11] *Ibid.* xiii.
[12] Cushing, *op. cit.* 2.549, 560, 607, 637, respectively.

enced bibliographer, had collated and described a great many of the books.

A note, written in his last illness, stated that he hoped his catalogue might be completed, if possible, before the books went to Montreal:

> It would be a great disappointment to me not to see the catalogue prepared in the sections in which I have divided the Library, but it is a big job and much remains. There is no reason why this library should not be kept in sections. . . . I should dearly love to see my Bibliotheca Prima idea carried out. . . . It was a great scheme and I had good fun over it.[13]

Lady Osler made possible the carrying out of the task by the editors, but she herself was not destined to see it quite completed. As for the huge volume itself, *Bibliotheca Osleriana* contains 7787 entries. Bibliotheca Prima lists 1702 volumes under nine sub-heads: The Beginnings, The Old Civilizations, The Greek, The Arabians, Middle Ages, Sixteenth, Seventeenth, Eighteenth, and Nineteenth Centuries. Bibliotheca Secunda has 2596 books, the largest number; Litteraria, 1311; Historica, 956; Biographica, 297; Bibliographica, 538; Incunabula, 105; and Manuscripts, 163. In the last named "Osleriana" are included thirty manuscripts of Osler's own writings which have, with a few exceptions, been identified and arranged since his death. Among the odd bits in these Oslerian manuscripts are requests that notes about his private cases be destroyed, with the exception of ten from each year. The selections, with patients' names erased, have been made by Dr. Thomas McCrae, but these are "not for the present to be consulted"; nor are his day-books and accounts, 1874-1919 (except for Vol. 1), to be consulted "for fifty years, at least," from November 1919.

A few examples from entries under various books may be cited here to show the "essay" flavor pervading even this *catalogue raisonné*. Under Oliver Wendell Holmes's *The Guardian Angel,* Osler's notation reads:

> The New England doctor has been well drawn by Holmes in this novel. We are sorry not to have more of Dr. Lemuel Hurlbut, on whose pilulae compositae and sal polychrest five generations of Oxbow villagers had thriven. . . . The doctor in American fiction is strong only in New England, where he has reached a position difficult to match elsewhere. . . .

And of George Eliot's *Middlemarch*:

> Ask the opinion of a dozen medical men upon the novel in which the doctor is best described, and the majority will say "Middlemarch." Lydgate is at once

[13] Osler, *Bibliotheca Osleriana* ix-x.

an example and a warning. He illustrates an attempt to break tradition and custom in a provincial town by doing general practice and yet not dispensing drugs. . . . The warning in his case is plain—not to marry a fool with a pretty face! We follow his wrecked career with pathetic interest. An unmitigated calamity, his marriage ruined his intellectual life in a soul-wasting struggle with worldly annoyances. . . .

Of Anthony Trollope's *Doctor Thorne,* Osler writes:

In this novel Trollope sketches incidentally the transition in the English profession from the apothecary to the general practitioner. . . . I love Dr. Thorne for his theory as to the happiness of children—"he argued that the principal duty which a parent owed his child was to make him happy." Wise man![14]

Finally one can cite two entries which the editor included and which enhance the delight of the reader who refers to the *Bibliotheca* on occasion. The first appears under Mennessier de la Lance's *Essai de bibliographie hippique, donnant la description détaillée des ouvrages publiés ou traduits en Latin et en Français sur le cheval et la cavalerie, avec de nombreuses biographies d'auteurs hippiques,* Paris, 1915-1917.

Sir William Osler, who regarded this work and Ferguson's *Bibliotheca Chemica* as models of what he called bio-bibliography, begins his review (in the *Veterinary Review* 2[1918] 1-4) thus: "Not naturally dry, bibliography is too often made so by faulty treatment. What more arid than long lists of titles. . . . What more fascinating, on the other hand, than the story of the book as part of the life of the man who wrote it—the bio-bibliography!"

The second accompanies Sir Ernest Clarke's *The Medical Education and Qualifications of Oliver Goldsmith*:

After graduating B. A. at Dublin in 1749, Goldsmith studied medicine at Edinburgh, Leyden, and Padua. The doubt as to the source, or even the genuineness, of his medical degree has not been altogether removed by the finding reported here at p. 94 [of the *Proc. Roy. Soc. Med.*, vol. 7, Sec. Hist. Med., 1914], of the following entry in "Jackson's Oxford Journal" of 18 Feb., 1769: "Yesterday Oliver Goldsmith, Esqr., Batchelor of Physick in the University of Dublin, author of 'The Traveller' . . . was admitted . . . to the same Degree in this University." No record of the Dublin M. B. has been found. Dr. Crawfurd, after weighing the evidence, still shares Macaulay's scepticism.[15]

The *Bibliotheca Osleriana* furnishes splendid evidence of the wide background underlying Osler's various literary researches. It gives nu-

[14] *Ibid.* Nos. 4950, 4770, 5526, respectively. It might be mentioned here that the late Dr. Logan Clendening did not share Osler's opinion of Lydgate in *Middlemarch;* in his *Handbook to Pickwick Papers* (New York, Alfred A. Knopf, 1936) 129, Clendening says that Lydgate "hasn't a trace" of "the spirit of practice." The mention of Anthony Trollope reminds one that he was the author of *The Fixed Period* (also included in the *Bibliotheca*), which inspired Osler's unfortunately misunderstood address of the same name.

[15] *Ibid.* Nos. 7208 and 4869, respectively.

merous examples of his critical abilities, and shows at the same time the unbelievable extent of his reading. Such a breadth of learning is of course necessary to the profitable carrying on of such literary investigations as Osler's, whether concerned with medicine or not. Even so, the vast knowledge requisite to a proper and significant study of bibliography is surprising when found in a man burdened with so many duties as this busy physician.

## II

After the publication of his *Practice of Medicine* in 1892, Osler did a good deal of non-professional reading during the summer. On December 14th of that year he read before the Johns Hopkins Historical Club a paper which was the outcome of this reading, "Physic and Physicians as Depicted in Plato." The club, during the year 1891-92, had mapped out a program which had dealt largely with a systematic study of Hippocratic writings, and it was the intention of the group to deal in 1892-93 with Galen. This, we are told, "almost proved the undoing of the society—Galen was too colossal—so after nibbling at him the club again took refuge in such miscellaneous subjects as the mood of its members suggested." Osler, who agreed with Sir Henry Maine that "nothing moves in the world that is not Greek in origin,"[16] and early came under the inspiring guidance of the Greek thinkers, throughout his life was the champion of Greek ideals. Exactly when he was introduced to Plato —and who did the introducing—even Cushing is forced to guess. But he cannot be far afield from probability when he says it was during those evenings when Osler had browsed in James Bovell's library. Nevertheless, by 1892, Osler had come to quote Plato as often as his eternal favorite Sir Thomas Browne. Both his essays of this period and the textbook just completed show the influence of the great Greek. The latter book has as its motto Plato's definition of the Art of Medicine: "And I said of medicine, that this is an art which considers the constitution of the patient, and has principles of action and reason in each case."[17]

In writing his esssay on Plato for the Historical Club, Osler chose, as a topic closely related to Hippocrates, the writings of his contemporary, who gives interesting and revealing sidelights on the conditions of medi-

[16] Cushing, *op. cit.* 1.370.

[17] Quoted from Plato's *Gorgias* in William Osler, *The Principles and Practice of Medicine* (New York 1892) vi. On this page Osler also quotes from the first Aphorism of Hippocrates: "Experience is fallacious and judgement difficult."

cine in the fourth century B. C. As an exclusive source, Osler used the third edition of Jowett's translation of the *Dialogues of Plato,* which, bought in London, Cushing further surmises "had in all probability been devoured on the steamer."[18] The essay, containing innumerable quotations from that edition seems to have, in fact, less of Osler than Jowett.

> So much of the daily life of a civilized community relates to problems of health and disease that the great writers of every age of necessity throw an important side-light, not only on the opinions of the people on these questions, but often on the condition of special knowledge in various branches. Thus a considerable literature already illustrates the medical knowledge of Shakespeare, from whose doctors, apothecaries, and mad-folk much may be gathered as to the state of the profession in the latter part of the sixteenth century. So also the satire of Molière, malicious though it be, has preserved for us phases of medical life in the seventeenth century.[19]

And Osler further goes on to point out that George Eliot also has given us everyday details of struggles in nineteenth-century medical practice. Strictly medical writings are peculiarly silent on this material found in Molière, Shakespeare, Eliot, and others.

The great idealist Plato, and Aristotle, the realist "who has swayed the masterminds for twenty-two centuries," give us much about Greek physic and physicians. Osler restricted his paper to Plato, giving in the first part his physiological and pathological speculations. The essay is next concerned with numerous interesting allusions to, and analyses drawn from, medicine and physicians. Quotations fill the pages, and in the second section are given the accounts of the origin of modern medicine, the relations of the arts of medicine and gymnastics. Hippocrates, though a contemporary of Plato, is only twice referred to in the *Dialogues,* although a glimpse of the method of diagnosis derived from personal observation is, Osler thinks, "possibly of the great Hippocrates himself."

In this essay there is quoted in full one of the most celebrated passages with a medical interest "in which Socrates professes the art of a midwife practicing on the souls of men when they are in labour, and diagnosing their condition, whether pregnant with the truth or with some 'darling folly.' "[20]

The social status of physicians—the private practitioners and the State practitioners—may also be gathered from Plato; as well as points

[18] Cushing, *ibid.*

[19] William Osler, "Physic and Physicians as depicted in Plato," *Aequanimitas with other Addresses* (Philadelphia, P. Blakiston and Sons, 1906) 49.

[20] *Ibid.* 65.

bearing on the advanced condition of medical literature; the medical symptoms narrated in the scene after Socrates drank the hemlock in prison, and the reverence felt for the representative of the great Healer. And the final words were the eulogium of Professor Jowett, ending: "So, partly trifling but with a degree of seriousness, we linger around the memory of a world which has passed away."[21]

## III

Following the Silliman Lectures which Sir William gave at Yale in 1913, his interest in medical incunabula grew apace, and when he made his presidential address before the Bibliographical Society on January 19, 1914, it was only fitting that he should have chosen as his topic "The Earliest Printed Medical Books." The essay concerns the influence of printing from its beginnings until 1480 upon the art of medicine—a period, it is said, "of greater typographical than scientific interest."[22]

Beginning with the statement that "the earliest books or incunabula possessed a special interest of their own as illustrating the origin and evolution of the art which more than any other had set free the mind of man,"[23] Osler showed how the individual books or their leaves told their family history, genealogy, and wanderings.

Despite the fact that a new discovery or process in practical science makes more progress in one decade than a new thought does in ten, the copyist survived long after the introduction of printing. However, in 1480, twenty-five years of printing had seen 111 printing houses and 350 printers at work.

In the fifteenth century there were three great centers of medical study—Montpellier, Paris, and northern Italy—but there was no outstanding name associated with them. Arabic medicine was supreme, Avicenna and Rhazes being more quoted by Ferrari in that period than Galen and Hippocrates; and of 182 works before 1480 to which Osler referred, 6 were classical, 8 Arabic, 23 medieval, and 30 of the fifteenth century. The Maintz calendar, published in 1457, was the first piece of

[21] *Ibid.* 76.

[22] Cushing, *op. cit.* 2.394.

[23] William Osler, "The Earliest Printed Medical Books," *Brit. Med. Jour.* 1 (January 1914) 205. This article is an abstract of the actual address: the complete essay was published in his *Incunabula Medica*: *A Study of the Earliest Printed Medical Books*, 1467-1480 (London, The Bibliographical Society, 1923), along with a bibliography of 217 books of that period. This list, begun by Osler, was completed by V. Scholderer of the British Museum after Osler's death. Cushing says Sir William was led "into difficulties which only the bibliographers of incunabula can appreciate."

medical printing and had to do with the phases of the moon or planets favorable to bleeding. There were no editions of Galen or Hippocrates, whereas Aristotle's *De Animalibus* appeared in 1476 and Dioscorides in 1478.

Eight Arabic authors with 26 editions showed the domination of the Arabs, and in spite of the Arabic-Greek controversy between Paris and Montpellier, the *Canon* of Avicenna held its own until the seventeenth century. "The Arabs contributed nothing to medicine or anatomy, but their heavy hand was still seen in the British Pharmacopoeia."[24]

Of the medieval writings, Osler mentioned *Flos Medicinae,* by which name the famous *Regimen Sanitatis* is often known—a book of which there are nearly 250 known editions. From this book came the couplet:

> Joy and Temperance and Repose
> Slam the door on the doctor's nose.

There also appeared in this period the first printed surgical books, the book of Albertus Magnus the great Schoolman and the *Physiognomia* of the great thirteenth-century scholar Michael Scott, who had made observations on the significance of dreams "with which Freud . . . would sympathize."

The first anatomical treatise, written in 1316 and printed in 1478, the work of Mundinus, was dominated by Arabic influence, and was inexact. Sir William mentions 12 small books on the plague, the first monograph on children's diseases (1472), on diseases of the eye (1474), a book on popular medicine (1477), and a 1472 volume "with a eugenic title and full of excellent common sense." Osler's last paragraph began with these words:

> Taking a survey of the period as illustrated by the output of the press, one cannot say that for the first twenty-five years of its existence printing did much, if anything, to free the profession from the shackles of mediaevalism. Not until the revival of Greek studies did men get inspiration from the true masters of science, and for at least two generations they were too busy looking for the fountains to explore for themselves the virtues of their waters.[25]

Dr. Fielding H. Garrison said of this essay, "Almost any other hand would have made a stupid dry-as-dust job of it, and again Sudhoff's remark comes back to me, that an essay of Osler's is worth many ponderous tomes of dry erudition."[26]

[24] *Ibid.* 1.205.
[25] Osler, *Incunabula Medica* 31.
[26] Cushing, *op. cit.* 2.395.

## IV

"To study the phenomena of disease without books is to sail an uncharted sea, while to study books without patients is not to go to sea at all."[27]

This, one of the most striking of Osler's metaphors, was made in his address, "Books and Men," given on January 12, 1901 at the dedication of the new building for the Boston Medical Library. The chief hall of this palatial edifice was named after Oliver Wendell Holmes, who had served for thirteen years as the first president of the Boston Medical Library Association and had made the library the repository for his books.

Dr. Osler began by telling how he had to go to Boston in 1876 for references unavailable at McGill, and he added that ever since then he could not speak of the value of libraries without seeming to exaggerate. Indispensable for the teacher and the worker, the library should have instructors to teach the young how to read. From his reading he drew on an old classification of four types of readers: "Sponges which attract all without distinguishing; Howre-glasses which receive and pour out as fast; Bagges which only retaine the dregges of the spices and let the wine escape, and Sives which retain the best onely."[28]

The general practitioner should avoid senility by controlling his everyday experience with reading. With how little reading a doctor can practice medicine is astonishing, "but it is not astonishing how badly he may do it." The utilization of the fresh knowledge of the journals was urged for the young physician. And a third class, to whom books are dearer than to teachers and practitioners—called bibliomaniacs by the profane—love books partly for their contents and partly for the sake of the authors. Their work is valuable for two reasons:

> By the historical approach alone can many problems in medicine be approached profitably. . . . But in a more excellent way these *laudatores temporis acti* render a royal service. For each one of us today, as in Plato's time, there is a higher as well as a lower education. The very marrow and fitness of books may not suffice to save a man from becoming a poor, mean-spirited devil. . . . The men I speak of keep alive in us an interest in the great men of the past and not alone in their works, which they cherish, but in their lives which they emulate. . . . This higher education . . . is the silent influence of character on character.[29]

Osler then expressed his desire to see a small band of Immortals set

[27] William Osler, "Books and Men," *Aequanimitas* 220.

[28] *Ibid.* 221.

[29] *Ibid.*

apart for special adoration—journals as well as books. While it is too early to select American medical classics, a list of twenty first-rank contributions deserve to be singled out. Ten of these are from New England, which in addition has given men, better than books, to other parts of the country: men who "carried away from their New England homes a love of truth, a love of learning and above all a proper estimate of the personal character of the physician." "Ambition," he went on to say, "usually in proportion to capacity, is as necessary in a profession as it is in a man, and the new library reflects credit not less on your ambition than on your capacity."[30]

This essay has been included in the present study because it best illustrates Osler's balance between the relative value of books and men. For one deeply attached to books—indeed "WO" was a book-collector and bibliographer—it would have been easy to lose focus; but here at the height of his career the great doctor, at the dedication of a house for books, was championing the man as of equal importance with his written word.

## V

During World War I, Sir William found great solace and refuge in his library, and his great collection was assuming generous proportions. To L. L. Mackall, the distinguished bibliographer of Savannah, Georgia, he wrote on February 7, 1916:

> The Library grows in spite of the hard and harassing times. Paré's *Anatomie Universelle* 1561 came in this week—not in England or S[urgeon] G[eneral's] L[ibrary]. Why so rare I cannot imagine. Malgaigne could only find two copies in France (1840). I suppose as one of the first anatomies to be printed in French the students used up the copies.[31]

This book must have delighted him, for he wrote a review of it for a new quarterly publication that was started in the Spring of 1917 by Dr. Francis R. Packard of Philadelphia—the *Annals of Medical History,* which later became a casualty of World War II. Osler's note, "The Anatomie universelle of Ambroise Paré," although brief, is nevertheless another good example of his wide literary interests.

Paré's early works were in the vernacular and so popular that they were "thumbed" away like school books; of the two extant copies of the *Anatomie universelle* of 1561, one, imperfect, is in the Sainte Geneviève

[30] *Ibid.* 225.

[31] Cushing, *op. cit.* 2.515.

Library and the other at Bar-le-Duc in private hands—at least such was the case when Osler wrote. Seeing a copy of this rarest of Paré's works advertised for sale in a Paris catalogue, Osler "sent a telegram and was delighted to get the book within forty-eight hours. The provenance is uncertain. It had come in with a number of unbound volumes and was sent to Chambolle-Duru, in whose famous morocco and unmatched gilding it is now adorned—a small octavo of 277 pages."[32]

Osler discussed the effect of the book on students of the day and mentioned the "gem of the book," a copper engraving of Paré at forty-five, the earliest known portrait. He ended on this hope: "The fitness of things demands that this copy should return ultimately to France to the great collection of the École de Médecine."[33] And a few years later, remembering his own words, "every book has its natural habitat," Sir William wrote in his will:

> To the Faculté de Médecine de Paris—
>
> Paré's Anatomie Universelle, 1561, of which no copy exists in their library or in the British Museum, and only two or three other copies are known.[34]

## VI

More interested in being connected with the Historical Sections of medical societies, Osler had in May 1918 declined the presidency of the Royal Society of Medicine. On May 15th, with the presentation of W. T. G. Morton's original papers to the Society, he made three remarks, which appeared in the *Annals of Medical History* under the title "The First Printed Documents Relating to Modern Surgical Anaesthesia."

> Although the idea of producing insensibility to pain during a cutting operation is mentioned in the Book of Genesis (2:21) and the word anaesthesia first occurs in Plato and was used by Dioscorides, controversy has raged over who introduced anaesthesia. Which illustrates the absence of true historical perspective and a failure to realize just what priority means in the case of a great discovery.[35]

Explaining why credit is not given to Dioscorides, Pliny, Hiotho the Chinese, Davy, Hickman, and others—all of whom knew of or used drugs or vapors to make patients insensible, he stated that no one man forced any specific method into general acceptance or influenced in any way surgical practice.

[32] William Osler, "The Anatomie universelle of Ambroise Paré," *Ann. Med. Hist.* 1 (1917) 424.

[33] *Ibid.* 1.425.

[34] Osler, *Bibliotheca Osleriana* xxx.

[35] William Osler, "The First Printed Documents Relating to Modern Surgical Anaesthesia," *Ann. Med. Hist.* 1 (1917) 329.

Credit for the development of surgical anesthesia into a world-wide procedure is due to William Thomas Green Morton, a Massachusetts dentist who demonstrated on October 16, 1846 the simplicity and safety of ether. "He becomes the true discoverer who establishes the truth; and the sign of the truth is the general acceptance. . . . In science the credit goes to the man who convinces the world, not to the man to whom the idea first occurs."[36]

Duplicate copies—the originals are nonexistent—of Morton's "Letheon" and "Remarks on the Proper Mode of Administering Sulphuric Ether by Inhalation" and a duplicate of *The Boston Medical and Surgical Journal* of November 15, 1846, which contains the first printed record of the new procedure by Dr. Henry J. Bigelow, and also Dr. John Collins Warren's account of the first operation at the Massachusetts General Hospital, were presented to the Royal Society Medical Library. The history of these four papers is recounted by Osler. Morton called his drug *letheon,* published a pamphlet, and gave the results of his experiments to the public through circulars he had printed. Letters of advice at first, they became four-page sheets including information from both America and Europe on anesthesia. He published other pamphlets: one in Paris, "Mémoire sur la découverte du nouvel emploi de l'éther sulphurique" (1847), "On the Physiological Effects of Sulphuric Ether and its Superiority to Chloroform" (1859), and a posthumous "The Use of Ether as an Anaesthesia at the Battle of the Wilderness."

In offering suggestions about the classification in libraries of literature on anesthesia along historical lines, Osler stated:

> I speak as an amateur. Doubtless experts could easily arrange a more comprehensive scheme. To separate in literature the quick from the dead is one of the functions of a well ordered library; but much that we carelessly regard as dead is magnetized into life when put in its historical relation. The plan here suggested —which could be applied in other directions—sustains that continuity, to the study of which this section is devoted. You remember the rings of Lucretius—well, there is a *vis a vincula librorum,* binding together books, a force just as potent as the *vis et vincula lapidis,* which supports the rings, and in the literature of anaesthesia this force is derived from the works here presented to the Library.[37]

## VII

Two later articles appeared in the *Annals of Medical History* under Osler's name: one was a review of F. Parkes Weber's *Aspects of Death*

[36] *Ibid.* 1.330.

[37] *Ibid.* 1.332.

*and Correlated Aspects of Life in Art, Epigram, and Poetry.* The book, a third edition of the avocational studies of one of the best known of London physicians, the reviewer said, "reminds one [in the appositeness and fecundity of its quotations] of Robert Burton, and nowhere in literature is to be found such a collection as that given in this section on the satires, sayings and epigrams relating to physicians and their art." And he closed with a personal note: "Friends have associated my name in a kind way with a good many books, but I have never before had a dedication which illustrates the *curiosa felicitas* of the scholar-student."[38] The dedication had read: "Gulielmo Osler, medico peritissimo."

Under "Historical Notes" in the same issue Osler has written a brief essay on Currie's "Journal," the manuscript of which had been sold with many letters to the Liverpool Public Library by Sotheby's on July 24, 1918. Dr. James Currie, the author, is remembered as the first editor of the collected works of Burns and as an early student of thermometry and hydrotherapy. The "Journal," a diary of a voyage from Nixonton, N. C., to the Island of St. Martin, September 19th to October 29th, 1776, illustrates the careful education of a Scotch lad and tells the horrid discomforts of a sea voyage. The manuscript of a letter written by 19-year-old Currie in defense of the Scotch in Virginia, is, Osler believes, more interesting and "full of common sense and well expressed." The other letters relate to Burns and his friends and were used by Currie in writing Burns's life. A few years ago, writes Osler, a collection of Burns's manuscripts—once Dr. Currie's—were sold by the Liverpool *Athenaeum,* to which they had been given by Currie's son. There was a public protest, but the Philadelphia purchaser gave them to the Burns Library at Kilmarnock.

In closing Sir William said that Weir Mitchell had a great admiration for Currie and "called my attention to his works, which he regarded as among the most valuable in English medical literature."[39]

A much longer essay, "Some Aspects of American Medical Bibliography," was delivered on June 10, 1902 at a meeting of the Association of Medical Librarians in Saratoga; but I must limit myself to only a few words about it and some other works of a bibliographical nature. Osler's paper, says Cushing, was "prepared with no less care for this small group

[38] William Osler, "Review of F. P. Weber's *Aspects of Death,*" *Ann. Med. Hist.* 2 (1919) 85.

[39] "Historical Notes," *Ann. Med. Hist.* 2 (1919) 81.

of people than it would have been for a larger audience."[40] In trying to impress his hearers with the wider aspects of library work in small medical libraries, he divided his essay into three phases: the book itself, the book as a literary record (i.e., the book for its contents), and the book in relation to the author. Osler's remarkably wide reading and his thorough knowledge of American medical writings are conspicuously evident in this paper: almost a hundred authors, books, and editions are mentioned, including the excursions of medical men into pure literature —Oliver Wendell Holmes, S. Weir Mitchell, George B. Wood *(First and Last),* Jacob Bigelow *(Eolopoesis).*

A strange place for a Regius Professor—but one to which Sir William was drawn by his many-sided literary interests—was the annual dinner, January 26, 1911, of the International Association of Antiquarian Booksellers at the Criterion Restaurant, Piccadilly, London. His address to the group appears to have been highly entertaining, for he said, in part, that he was about to have his revenge on them:

> Until I became mixed up with you I was really a respectable, God-fearing, industrious, earnest, ardent, enthusiastic, energetic student. *Now* what am I? A mental wreck, devoted to nothing but your literature. Instead of attending to my duties and attending to my work, in comes every day by the post, and by every post, all this seductive literature with which you have, as you know perfectly well, gradually undermined the mental virility of many a better man than I. . . . But I have the greatest satisfaction in feeling that I am going to have my revenge, and I noticed the first indications of it the other day. I saw in one of the catalogues "Osler's Principles and Practice of Medicine"—the first time I had seen it in an Antiquarian Catalogue. But just think what will happen in the next generation. . . . [The 125,000 copies will be] an awful bother and worry. . . . But when you think of the "System of Medicine" in seven volumes, seven volumes! each weighing several pounds; just think when that gets on your shelves—it will not be worth the paper! Then I know perfectly well I shall have had my revenge.[41]

During the years 1912, 1913, and 1914 Osler published in the *Canadian Medical Association Journal* his "Men and Books" series, a monthly contribution for 18 months on some 26 topics relating to medicine and its allied arts: "Les Collections artistiques de la Faculté de Médecine de Paris," "Jean Astruc and the Higher Criticism," "Two Frenchmen on Laughter" [Joubert and Bergson], "The Works of John Caius," "Gomperz's *Greek Thinkers,* "Aristotle," "Dr. Slop," and others.

For the first meeting of the new section of the History of Medicine of the Royal Society of Medicine, London, on November 20, 1912, Osler,

[40] Cushing, *Life* 1.580. The essay is reprinted in *Aequanimitas,* 307-326.

[41] *The Bookseller* (London, February 3, 1911) 144.

elected chairman, prepared a paper on "A Down Survey Manuscript of William Petty." By chance he had discovered in a purchase of Petty's manuscript letter-book a sheet with the famous survey of Ireland after Cromwell's invasion. The manuscript was presented to the Library of Trinity College, Dublin. Of Petty, "born out of time," Osler quotes Pepys, who had many notices of Petty in the *Diary*: "'In discourse methinks one of the most rational men that ever I heard speak with a tongue, having all his opinions most distinct and clear'—a judgment amply confirmed by all those who have studied the writings of this remarkable man."[42]

## VIII

Now, these essays, papers, studies, or whatever one wishes to call them have here been treated as literary investigations: perhaps one or two of them may be said to be "literary" in the usual meaning of that term, but all of them deal in a scholarly way with books. Early medical bibliography is quite as important in throwing light upon literary works of early authors as is special bibliographical or philological or historical investigation of any other sort.

Thus Chaucer's, Shakespeare's, or Burton's works receive a fuller understanding when studied with a positive knowledge of the condition of medicine in the fourteenth and sixteenth centuries. Much of Shakespeare's material is vastly clarified by the application of sixteenth-century medical lore to certain passages. The character of Hamlet may be fully understood only in the light of what Shakespeare could have known about medical subjects; and this is true of others also, like Lear, Richard III, and Othello. Further, it seems plain that no student of English philology can afford to overlook the significance of historic medical learning in interpreting the literature he studies. Just as Chaucer's works are filled to the brim with references to medical and alchemical practices of the fourteenth century, so are an enormous number of works in literature, from the earliest to the present time.

In Osler's *Creators, Transmuters, and Transmitters as Illustrated by Shakespeare, Bacon, and Burton,* we have the lover of good literature speaking more than the doctor. Rather than a medical, it is a literary essay—literary in the sense that it introduces a new critical idea. So far as can be determined, the thought contained in this essay is original with

[42] *Lancet* 2 (1912) 1504.

Osler. It is developed, not by quotations illustrating the ownership of the idea by earlier critics, but by a profusion of quotations from the works he speaks of, used in illustration of his theses. And these contentions are subjective material, derived from Osler's own contemplation of the subject and appealing to the inner sense of all his hearers.

Shakespeare is of course the "creator" in this essay; Bacon is referred to as the first of the great "transmuters"; and Burton the last of the great "transmitters." Osler himself could be fitted easily into both these latter categories: consider both the transmutation of nature's laws into human applications, to be found everywhere in his inspirational lay sermons and in his scientific essays; and the transmission of human thoughts in the great abundance of quotations throughout all his writings, even his strictly medical works.

Another essay in the present group that is largely literary is the one read at the opening of the library of the Boston Medical Association, on "Books and Men." The conditioning of the minds of men by books is a theme which might be called inspirational, but I have used it here to a certain extent because it was so endowed with bio-bibliographical illustrations. Its theme, again, deals not with cosmic truth, but with human, homely considerations at once poetic and practical.

Some of the other of Osler's papers here treated have had to do with criticism of a scientific sort (in the sense of literature as a science), and with philology (if it be granted that incunabulic bibliography is a necessary adjunct to philology). They represent the other side of Osler's literary concerns, that scholastic complement without which creation and interpretation are the empty mouthings of children. We might close by arguing that although an enormous amount of so-called "creative" criticism has been dumped upon the literary market and accepted by the reading public—much of it is thin and valueless, because it has not the necessary background of careful scholarship and profound reflection so well combined in Sir William Osler.[43]

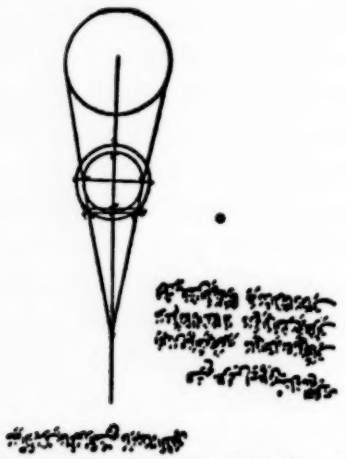
[43] It is a pleasure to acknowledge the assistance of Dr. Herman T. Radin of New York City, who patiently checked sources unavailable to me in Texas, where this article was written.

# Avicenna's Praise of Euclid

A. SÜHEYL ÜNVER*

EUCLID was a famous mathematician of the fourth century B. C. According to *Takvim-el-Tevarih* Euclid was an engineer in the famous old city of Sur in Syria,[1] but according to another source he came from the city of Sur near Jerusalem.[2] He owes his fame to his work on geometry. Euclid's geometry has been called "spiritual engineering expressed in physical form,"[3] and his name was often written with respect and honor on the blank pages and margins of books dealing with dogmatic and positive sciences.

The book he wrote is known by his name. According to an old story, the book of Euclid, i.e., the key to geometry, was written by Ablonius,[4] who lived long before Euclid. Whether this is true or not, with this book the name of Euclid has been transcribed in gold leaf among the immortals of science.



This poem by Avicenna, written in praise of Euclid, and the accompanying figure, are found in the Treasury Archives of the Museum at the Palace of Topkapi.

Avicenna writes thus in his biography: "I have fully understood the science of logic and the book of Euclid. With him[5] I read five or six figures out of Euclid, but the rest of the book I finished all by myself and was able to solve all its contents. Later I passed on to the reading of Magisthus."[6]

During an investigation at the Treasury Archives of the Museum at Topkapi, we found a geometrical figure drawn on hand-made glazed paper, together with a poem by Avi-

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[1] The historical calendar, in the general library at Süleymaniye, Section on Esad Effendi, no. 2234, year 5346.

[2] The library of Veliyeddin Effendi, no. 1552, *Selections from Euclid*. See also no. 989 in the section on Reis-el-Kitap Mustafa Effendi in the general library at Süleymaniye.

[3] The general library at Süleymaniye, Section on Esad Effendi, no. 3701.

[4] The book of Ablonius under the title "A Book of Engineering" was written in the Tower of Musul (Iraq) in the year 638 (1249). In 644 (1246) a new corrected edition came out. See the general library at Süleymaniye, division in the New Mosque, no. 803.

[5] Abu-Abdallah Al-Nasky, one of Avicenna's teachers.

[6] Avicenna's biography, "The Book of News of Different Grades of Physicians, as dictated by himself to his student, Abu Ubeid Geortany," and translated into Turkish by M. Sherafeddin Yaltkaya in 1937, under the title *The Works of Ibni Sina*.

cenna praising Euclid. He had written it as a supplement to his biography of Euclid. Over the figure is written in Arabic the statement that the figure represents an instrument which announces the end of dawn and the oncoming of the day. Following the explanation of its use are a few lines, written in praise of Euclid by Ibni Ali-bin-Sina, known in the East as Sheyl-ul-Reis (the head of the Sheiks).[7] A free translation of the Arabic verses follows:

> Euclid in sciences is so great that his name is synonymous with events happening in heaven and on the horizons. He is like a stairway whose steps are his figures, on which ascend passengers to endless heights. A person who makes his acquaintance is sure to rise to a high level. That level is ever so pleasant and the person who reaches it is ever so happy.[8]

This poetic praise of Euclid, written by Avicenna, is an expression of the value and the high esteem that Avicenna placed on Euclid. Indeed Euclid's work was considered the best book on geometry all through the Middle Ages by the Islamic and the Turkish worlds. Several famous scientists commented upon the book and added footnotes to it.

Fatih Sultan Mehmed, who conquered Istanbul on May 29, 1453 (according to the Arabic calendar, in the year 857), had a great interest in the positive sciences. His private library contained beautiful manuscript copies of the scientific works of the time. One of the best examples of these manuscripts is in the library of Feyzullah Effendi.[9] At that time scientific works in the Turkish, Arabic, and Persian languages were printed in foreign countries by the *efrentch*[10] who came to Turkey for commercial interests. Among the works published outside the country is a copy of Euclid printed in Rome.[11]

[7] In another copy of this poem, no. 797 in the library of the New Mosque, the first two couplets end differently. At the end of the first couplet is written "together and in the horizons" instead of "and all the horizons"; and at the end of the second couplet the words "by merits" occur, instead of "heights." Naturally, these alterations change the meaning of these two couplets entirely. Since the person who is responsible for these alterations is unknown, a translation of the altered form of the lines is not given here.

[8] These verses were freely translated into Turkish by the Late Hasan Fehmi Tugrul, general director of the libraries at Istanbul.

[9] See no. 1359 in the museum of the library of Ali Emiri Effendi: "The wise searcher and philosopher, the upholder of the nation and religion, Muhammad ben Muhammad Al-Tusi, of the writing of Euclid," and "the best of engineers Abi Ali-el-Hasan ben El-Hasan ben El-Haytham of the theories of Euclid." These two works are beautifully written and are in an elegant volume.

[10] The word *efrentch* used to mean any one of European origin, regardless of country or nationality.

[11] Copies of this edition are in the library at Manisa under the heading "The Principles of Euclid by Khuaja Naseer El-Deen Tusi," and in the section on Fevzi Pasha in the general library at Chorum. Attached to these copies is a printed order of Murat III, grandson of Süleyman the Magnificent, issued in 996 (1588). According to this order books brought into Turkey for sale would not be confiscated by the government authorities, unless they were books of a religious nature.

During his lifetime Avicenna was often attacked by his contemporaries. Thus in the edition in the library of Veliyeddin Effendi (registered under the number of 3191, document 176) attached to Avicenna's poetic praise of Euclid, the following lines occur: "Do you not see how handsome Euclid is, the owner of a sable coat?" In trying to find the meaning of these lines, I read the whole poem and found that in the last couplet the writer had attacked Avicenna. This last couplet is not quoted here, because the writer in insulting Avicenna used words not fit to be transcribed here.

With the present article I hope that Avicenna's appreciation of Euclid will be fully realized by the Western world and that Avicenna will be a source of reference for students of Euclid's works.

# A Critical Study of the Origins and Early Development of Hypodermic Medication

NORMAN HOWARD-JONES*

*Il m'est arrivé de voir plusieurs fois, dans mes recherches bibliographiques, les affirmations d'un auteur transformées en négations par une série de traductions et d'analyses.*

Paul Bert, 1878[1]

## 1. Introduction

THE effects of the bites of venomous snakes and insects pointed clearly to the possibility of the introduction of drugs through punctures in the skin. In primitive societies, the application for therapeutic purposes of plant and animal products through cutaneous incisions is practiced (Chauvet, 1936), and the use of poisoned arrows may be regarded as a crude precursor of hypodermic and intramuscular medication. Nevertheless, it was not until the nineteenth century that hypodermic medication was initiated, although percutaneous medication had been empirically employed since ancient times in the form of baths, ointments, and cataplasms. The hypodermic was in fact the last of the commonly employed routes to be adopted: the inhalation of medicated vapors; rectal, intra-urethral, intra-uterine, and intravenous injections; and the injection treatment of hydrocele all preceded hypodermic medication. Intravenous medication[2] had been practiced intermittently, both in animals and in man, since the seventeenth century.

It was not until just before the beginning of the nineteenth century that methodical attempts were made to employ the skin as a medium for the absorption of medicaments. Before 1800, Brera systematically employed percutaneous administration of drugs by inunction, to which he gave the name "anatripsology." Chrestien (1804) developed the technique under the name *méthode iatroleptique,* to which the term *cipnoische Medizin* (Eulenburg, 1880) was also applied.

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[1] Having learnt the truth of Paul Bert's assertion, the author has refrained from translating into English quotations from French and German writers.

[2] The history of intravenous medication is discussed by Macht (1916) and by Villaret & Moutier (1933). An excellent account is to be found in *Ciba Zeitschrift* No. 100 (March 1946). This includes a short section on subcutaneous injection, which is not discussed here as it was received after the completion of the present study.

In 1828 Lembert published a monograph of 124 pages on the ***méthode endermique,*** which had previously (1825) been described by Lesieur. This technique consisted of the application of a vesicant to remove the epidermis, after which the drug, usually one of the recently isolated alkaloids, was applied to the denuded cutis as a powder, solution, or ointment. The method won considerable popularity and was adopted by Trousseau among others. Eulenburg 1865 suggested that it should properly be described as the emplastro-endermic (***emplastro-endermatische***) method.

In 1809, Magendie reported the lethal effects in dogs of a Javanese arrow-poison, of which the active principle was later found to be strychnine. Magendie administered the poison by applying it to the extremity of a wooden barb, which was thrust into the dog's buttock. In spite of the interest aroused by these and subsequent experiments demonstrating the absorption of poisons via the subcutaneous and muscular tissues, it was not until nearly thirty years later that drugs were administered therapeutically by mechanical penetration of the skin.

It may be noted in passing that Sir Robert Christison in 1831 suggested that whales might be killed by fastening a phial of prussic acid to a harpoon "in such a way that the phial should break when a strain was put upon the harpoon after entering the body of the animal" (Christison, 1886). This method would appear to have been used, although precise information on the results is lacking.

### 2. SUBCUTANEOUS ADMINISTRATION OF SOLIDS

*a. Lafargue's "Inoculation."* On 26 September and 3 October 1836, two short letters from a Dr. G. V. Lafargue (1836a) of St. Emilion were read to the Paris Académie des Sciences, and on 4 October 1836 a more detailed communication was submitted to the Académie de Médecine by the same author. In these letters Lafargue describes how, following a study of the effects of the application of drugs to the skin denuded of its epidermis, he had conceived the idea of introducing morphine and its salts under the epidermis by means of a vaccination-lancet. The extremity of the lancet was dipped in morphine, preferably moistened with a little water, and then forced almost horizontally beneath the skin, retained there for a few seconds, and then withdrawn (Fig. 1). Lafargue notes that a small papule with a pink areola developed after 30 seconds, subsequently increasing in size to reach a maximum in 1 hour.

Although regarding his "inoculation" primarily as a form of local medication, Lafargue asks:

> La morphine ainsi transmise dans le torrent circulatoire, ne peut-elle excercer une influence générale sur l'économie et retenir sur les organes éloignés?

This question he answers by reference to the results of an experiment performed upon himself. Thirteen punctures were made in the skin of his forearms, employing in all a total of 1/4 grain morphine. After an hour he was overcome by the desire for sleep. Nevertheless, he does not appear to have been impressed by the possibilities of inoculation as a method of systemic medication, for he insists that it is in those cases, especially of superficial neuralgia, in which local effects are required, that the method will be of value. He relates the cure of his own facial neuralgia by multiple punctures along the course of the affected nerve, adding:

> Cette méthode . . . permet d'attaquer la douleur sur tout le trajet du nerf à la fois, de la suivre jusque dans les petites ramifications. . . .

The use of the method in chronic articular rheumatism is also proposed.

At the session of the Académie de Médecine the following week (11 October), a second communication from Lafargue was presented. This is almost entirely devoted to a study of the cutaneous reaction following inoculation of morphine and preparations of crude opium, which Lafargue claims to be sufficiently specific to serve as a test of great forensic importance, and which he describes in considerable detail. The reaction was elicited by the considerable dilution of opium represented by a 1 per cent solution of Sydenham's laudanum (=0.05% opium). In cases of suspected opium poisoning, Lafargue proposes, the gastric contents could be tested for opium by means of this reaction.

On 27 December, Martin-Solon (1836), a physician of the Hôpital Beaujon, reported to the Académie de Médecine the results of an investigation of Lafargue's technique made by himself and his interne, "sous le double point de vue de la médecine légale et de la thérapeutique." He confirms Lafargue's observations on the cutaneous reaction to opiates, but points out that similar appearances were obtained by inoculation of belladonna, strychnine, and a mixture of gastric juice and chyme. Discounting the forensic significance of this cutaneous reaction, he wisely adds: "Gardons-nous des demi-preuves en médecine legale."

Reporting on his therapeutic trials, Martin-Solon says that a woman

with "céphalalgies nerveuses" appeared to be cured but experienced somnolence. A second patient with rheumatic pains noticed only somnolence with no relief of pain. A third, with arthritis, experienced neither relief nor somnolence. Experiences with other drugs in other conditions were similarly disappointing. Summing up his results, Martin-Solon points out that "il n'y a point analogie" between Lafargue's inoculation and "inoculation du vaccin." The latter procedure was designed solely to produce an affection analogous to that which it prevented, while in the former a heterogeneous agent was introduced "pour détruire une affection avec laquelle cet agent n'a rien de commun."

Has Lafargue's method any advantages, differing as it does from "les méthodes thérapeutiques et endermiques" only by the technique? Martin-Solon answers unenthusiastically, "Il est probable que dans plusieurs affections locales, on en retirera quelques avantages." However, he adds, the type of affection for which the new technique is advocated is such as to require caution in judging the results. Inoculation of morphine hydrochloride had been prescribed for two patients with "douleurs rhumatismales sur le trajet du sciatique." On the following day they were free from pain—but the treatment had not been carried out!

> Si on les eût pratiqués, nous aurions compté ces faits parmi les succès obtenus. C'est ainsi que trop souvent on accrédite les merveilles thérapeutiques.

Martin-Solon's analysis of Lafargue's claims has been discussed in some detail because of the scientific discrimination that it reveals. This is in striking contrast to the uncritical enthusiasm of later advocates of various techniques of hypodermic medication and, indeed, to that of many modern therapists. (It is necessary only to recall the plurality of treatments at present advocated for sciatica.) Martin-Solon concludes by remarking that "le travail de M. Lafargue n'est pas sans intérêt" and moving that his communications be deposed in the archives of the Académie.

Writing 25 years later, Lafargue refers to his original communications as having merited the distinction of a favorable report from Martin-Solon!

*b. Lafargue's Acupuncture-Inoculation.* In 1847 Lafargue published a paper in which he reported a modification of his technique. He had noticed in some of the cases treated by his original method an instantaneous relief of pain—before the morphine could have had time to act. This led him to the idea of combining acupuncture and inoculation, for

which purpose he devised a long needle with a deep gutter which was filled with morphine paste.

*c. Lafargue's Enchevillement* (*Pellet-Implantation*). However, with the publication in 1859 of Béhier's memoir (see section 5, below), the hypodermic injection of solutions became widely diffused in France and elsewhere in Europe, and in 1861 Lafargue published details of a new method which, in his opinion, was superior to all others. He had, he says, maintained silence about the improvements that he had made in his own method, while observing the experiments of others.

> Aujourd'hui que la lumière me semble devoir être faite sur la valeur pratique de la méthode de Wood, je crois devoir rompre le silence, car le progrès n'est aussi complet que ses fauteurs l'ont espéré.

It was important that a new method should be simple. Did the hypodermic syringe fulfill this condition? It was costly, deteriorated rapidly, and its handling was "minutieuse et délicate." For all these reasons, the syringe was to be found in the hands of only a few doctors; "il n'est pas entré dans le domaine de la pratique courante." The ordinary "clientèle" of both town and country had not benefited by the method "et n'en bénéficiera assurément jamais."

Lafargue then announces that he wishes to obviate these faults, and to endow therapeutics with a weapon which is at the same time efficacious, easy to use, and of a moderate price. Lafargue's new method, which he describes as *inoculation hypodermique par enchevillement* ("plugging"), certainly fulfilled some of these criteria, for apart from the medicated pellets (*chevilles*) used, the only equipment required was a darning needle (*une modeste aiguille de bas*). In preparing the pellets equal parts of gum arabic and distilled water were mixed to form a thick mucilage. 1-2 centigrams of the mucilage were then mixed with 5 centigrams of atropine sulphate (or other active ingredient) and 4 centigrams of sugar were added to form a mass of the consistency of a pill. This mass was then rolled into a cylinder 12.5 cm. long, and narrower than the bore of the needle. The cylinder was then divided into 25 parts, each of 5 mm. (misprinted as 50 mm.) in length and containing 2 mg. of atropine. Lafargue pays tribute to Prosper Barade, a young pharmacist of Saint-Emilion, for his ingenuity in making the cylinders.

The pellet was implanted subcutaneously by taking the skin over a painful area between index and thumb, inserting the needle obliquely through the skin to a depth of 60-70 mm., withdrawing the needle, and

inserting the pellet with the fingers or with tweezers. The puncture was covered with collodion, a piece of stamp-paper(!), or several other specified materials, and thus found itself "emprisonné." Tissue fluids began to dissolve the pellet,

> les nerfs, siège de la névralgie, en sont impressionés, et en moins d'un quart d'heure s'observent les phénomènes locaux de sédation, et plus tard des symptômes généraux, si la dose du médicament a été un peu exagérée.

*d. Lafargue's Seringue Sèche.* In patients in whom there was a tendency for the puncture to close before insertion of the pellet, Lafargue used a sort of implantation-gun which he describes as a "dry syringe" (*seringue sèche*). This instrument was a cannula 10 cm. in length with an aperture at one side. It was inserted through the puncture previously made with the needle, and the pellet was placed into the lumen through the lateral aperture. The needle was then used as a ramrod to drive the pellet into the subcutaneous tissue. Referring again to the hypodermic syringe, Lafargue says that he has found it simpler and better to inject *cylindres médicamenteux* with his *seringue sèche.* Finally he points out the advantages of his original and simpler technique of inoculation.

*e. Brun's Implantationsnadel.* Bruns (1869) with some justice regards Lafargue's original inoculation as a form of endermic medication, the implantation of pellets being a truly hypodermic method. For the latter procedure he designed an *Implantationsnadel* (Fig. 2), which was a considerable improvement upon Lafargue's *seringue sèche,* and which, he claimed, simplified the operation of implantation.

The medicated pellets were placed in the guttered aperture at the proximal end of the needle. Distal displacement of the button operated the ramrod, the extremity of which can be seen in the aperture. In this way, the pellet was propelled through the hollow needle into the subcutaneous tissues. The needle was 5-6 cm. long, and the handle, which was of ivory, 9-10 cm. For the pellets (*Arzneistäbchen*), he proposes the name *Bacilli medicamentosi.* Bruns refers to one of the possible disadvantages of the method as being slow and uncertain absorption, but he adds that this has not been the case in his clinic.

There is no suggestion either by Lafargue or by Bruns that advantage should be taken of the implantation technique to provide a depot from which sustained and gradual absorption could take place.

*f. Crombie's Impregnated Setons.* The subcutaneous implantation of drugs never seemed to gain general acceptance (except, perhaps, in

the use of caustics in malignant disease), and even Lafargue's advocacy of his methods rests largely upon the alleged technical defects and high cost of early hypodermic syringes.

These faults seem still to have been operative as late as 1873, in which year Crombie writes:

> In consequence of the dearness and delicacy of the syringe, the hypodermic application of morphia . . . has not come to be so extensively used as it otherwise would. I have devised a singular and much less expensive method, by coating a fine silk thread with the required quantity of morphia, which when introduced in the manner of a seton, by means of a fine needle, with the eye close to the point, enables us to deposit morphia beneath the skin, by slowly drawing the thread through the opening.

The thread was dipped in water before insertion, and the "suppositories" could be commercially obtained from Messrs. Mackie of Bouverie St., London, in packets of a dozen, with needles, and in doses ranging from 1/6 to 1/2 grain morphine.

Eulenburg (1880), confusing "silk" with "silver," refers to Crombie's method as involving the use of "mit Morphiumlösung bestrichenen Silberdrähten." However, Crombie's suggestion was not original, as Lafargue had described an identical technique in 1847.

*g. Langenbeck's Flächenimpfung.* Eulenburg (1880) describes a modification of Lafargue's inoculation introduced by Langenbeck, and named by its author *Flächenimpfung* (surface inoculation). Langenbeck applied drugs in solid form to incisions of the skin. Strong irritants (cantharides or croton oil) were then applied to the incision, which became converted to an ulcer. Medicaments were placed in the crater of the ulcer, and the whole was covered with adhesive plaster. Fluids were similarly applied as soaked pledgets of cotton-wool. *Flächenimpfung,* a development of inoculation, is mentioned here for convenience, although it was hardly a subcutaneous method.

By 1880, however, Eulenburg is able to dismiss all such methods, including Lafargue's pellet implantation, as completely outmoded by hypodermic injection of fluids, and he refers to objections to the latter as quite unfounded.

*h. The Rediscovery of Implantation in* 1937. The technique of subcutaneous implantation of medicaments fell into disuse until it was revived again, with every appearance of novelty, 76 years after its introduction. In 1937 Deansley & Parkes described a technique of implantation of compressed tablets. This procedure was employed in animal experi-

ments with steroid hormones, and was applied clinically by Bishop in 1938. In 1939 Vest & Howard described an "injector" for the subcutaneous implantation of pellets of androgenic steroids. This "injector" was essentially a modern version of Bruns' *Implantationsnadel* and the cruder *seringue sèche* of Lafargue. The purpose of the report by Vest & Howard was " . . . to present a new[!] technic for the subcutaneous implantation of solids such as pellets of pure crystalline androgenic substance by the use of an 'injector' instrument."

At the end of their paper they say:

> It is possible that such an instrument as we have developed and presented here might be applicable to injection of other solid medicinal materials.

In fairness to those responsible for the modern use of the implantation technique, it should be added that Lafargue at no time appreciated its possibilities as a means of securing the slow and continuous absorption of a medicament into the bloodstream.

### 3. SUBCUTANEOUS ADMINISTRATION OF FLUIDS

*a. Rynd's Retractible Trocar.* In 1845 a new treatment was described by Rynd, of the Meath Hospital and County Infirmary, Dublin. A woman of 59, who from Rynd's description appears to have been suffering from trigeminal neuralgia, was admitted to the hospital on 5 May 1844.

> On the 3rd of June a solution of fifteen grains of acetate of morphia, dissolved in one drachm of creosote, was introduced to the supra-orbital nerve, and along the course of the temporal, malar, and buccal nerves, by four punctures of an instrument made for the purpose.

Rynd here gives no description of the instrument used. He claims that relief was practically instantaneous, and that the patient slept better that night than she had done for months. The operation was repeated one week later, and after a few weeks more in hospital the patient was discharged well and did not return.

In a second case, that of a man of 28 with symptoms typical of sciatica, "fluid was introduced" (10 grains of morphine acetate in 1 drachm of creosote) in two sites along the course of the sciatic nerve. This was repeated after 3 days, and the patient was discharged "well" 13 days after the second injection.

It was not until 1861, by which time the use of the hypodermic

syringe was well known, that Rynd published a description of his instrument (Fig. 3). It was not a syringe, but an elaborate trocar and cannula. Pressure on a catch caused retraction of the trocar, and this permitted descent of the fluid which had been poured into the lumen of the cannula, whence it reached the tissues by gravity. Rynd's own description of his instrument (1861) follows:

> The canula (A) screws on the instrument at (B); and when the button (C), which is connected to the needle (F), and acted on by a spring, is pushed up (as in Fig. 2), the small catch (D) retains it in its place. The point of the needle then projects a little beyond the canula. The fluid to be applied is now to be introduced through the hole (E), either from a common writing-pen or the spoon-shaped extremity of a silver director; a small puncture through the skin is to be made with a lancet, or the point of the instrument itself is to be pressed through the skin, and on to the depth required; light pressure now made on the handle raises the catch (D), the needle is released, and springs backwards, leaving the canula empty, and allowing the fluid to descend.

Rynd (1861) claims:

> The subcutaneous introduction of fluids, for the relief of neuralgia, was first practised in this country by me, in the Meath Hospital, in the month of May, 1844.[3]

*b. Alexander Wood's "New Method of Treating Neuralgia."* In 1855 Dr. Alexander Wood (Fig. 5) of Edinburgh published the first account of the subcutaneous injection of solutions of drugs for therapeutic purposes. He was apparently unaware of Rynd's earlier (1845) publication, which in any case gave no details of the procedure adopted. Wood opens his paper by reference to improvements in the treatment of neuralgic affections consequent on Valleix's observation that sensitive points could be identified along the course of affected nerves, and on Valleix's advocacy of blistering over such points and, in some cases, the endermic applications of morphine (emplastro-endermic method, see Section 1). Wood says that he has "almost invariably" used a morphine-containing paste to dress the blistered surface, but that it has often occurred to him that the direct application of a narcotic to the nerve or its immediate neighborhood might be more efficacious. Describing how he came to use a syringe subcutaneously, he tells us that he had made several attempts

> to introduce morphia directly by means of puncture needles and otherwise, but without success. Having occasion, however, about the end of 1853, to endeavor to remove a naevus by injection with the acid solution of perchloride of iron, I procured one of the elegant little syringes constructed for the purpose by Mr. Ferguson

[3] In his original (1845) publication, the month of June is given.

of Giltspur Street, London. While using this instrument for the naevus, it occurred to me that it might supply the means of bringing some narcotic to bear more directly than I had hitherto been able to accomplish on the affected nerve in neuralgia. I resolved to make the attempt, and did not long lack the opportunity.

Wood then describes how an elderly spinster, suffering from "cervico-brachial neuralgia," had been unable to sleep, the usual remedies, with the exception of opium, for which she had an idiosyncrasy, having failed.

Under these circumstances, I resolved to put into practice the plan which I had so long revolved in my mind. Accordingly, on November 28th, I visited her at 10 p.m. to give the opiate the benefit of the night. Having ascertained that the most tender spot was the post clavicular point of Valleix, I inserted the syringe within the angle formed by the clavicle and acromion, and injected twenty drops of a solution of muriate of morphia, of a strength about double that of the official preparation.

Pereira (1857) gives the Morphiae Muriatis Solutio of the Edinburgh Pharmacopoeia as containing 1 grain of morphine hydrochloride in about 106 minims of solution. Taking a drop as equivalent to a minim, twenty drops of the solution used by Wood would therefore contain about 2/5 grain of morphine. Ten minutes after receiving this dose, the old lady complained of giddiness and confusion of ideas, and in 1/2 hour her pain had subsided. Wood visited her again on the following morning at about 11.00 a.m., and he was

a little annoyed to find that she had never wakened; the breathing was also somewhat deep, and she was roused with difficulty. Under the use of somewhat energetic stimuli, however, these symptoms disappeared, and from that time to this the neuralgia has not returned.

Wood then describes eight further successful cases, and adds particulars of two others treated by a Dr. Thomas Wright, F.R.C.P. In all except Wood's first case, "Battley's sedative solution" was injected.

Towards the end of his paper, Wood refers to Lafargue's first technique:

Inoculation . . . has been proposed by M. Lafargue St Emilion [*sic*], to be extended so as to secure the application of remedies. This method was brought before the Westminster Medical Society in February 1837, by Dr. Bureaud, but from the account given in the "Lancet" he does not seem to have been very successful, as only a slight local effect was produced.

Wood then refers to the report made in 1836 by Martin-Solon who, he asserts,

comes to the somewhat damaging conclusion that the effect produced was very much the same, whatever agent was inoculated, even when the experiments were made with agents as dissimilar as belladonna, strychnine, the gastric juice, chyme.

This is a most unfair comment, as Martin-Solon was in this context referring to the non-specificity of the cutaneous reaction which was suggested by Lafargue as a test for the presence of opiates. Wood reveals that he has engaged in experiments on "an improved apparatus" designed to cause less injury to the tissues.

The writer has been unable to trace an illustration of the Ferguson syringe, but in the museum of the Royal College of Surgeons of Edinburgh there is a broken glass syringe which is said to have been used by Wood (Fig. 4). This syringe is not graduated, as was the improved model that Wood describes in 1858, and, assuming its authenticity, it seems reasonable to regard it as an example of the original Ferguson syringe. A description and photograph of the syringe are given by Comrie (1931).

The barrel, piston-rod, and piston are of glass. Cotton-wool has been wrapped round the piston. The metal parts are tarnished, but are almost certainly of silver. The conical metallic extremity terminates in a male screw, upon which the hollow needle was fixed. Wood's reference to the improvements upon which he is experimenting indicates that the Ferguson syringe was not without defects and, particularly, that it traumatized the tissues.

Gaujot (1867) says that the Fergusson [*sic*] syringe is not sufficiently accurate, owing to the large size of the *canule*. Nevertheless, he pays tribute to Ferguson for having introduced "le principe ingénieux de la canule-trocart," the hollow needle with a sharpened extremity which could penetrate the skin while screwed on to the syringe.

Jousset (1865) describes the Fergusson [*sic*] syringe as having a hollow steel needle with a very sharp fluted extremity which, however, penetrated the skin with difficulty and caused pain. He also criticizes it on the grounds that it is badly made and that the barrel is not graduated. Currie (1887) says of the Fergusson [*sic*] syringe that the needle "being of too large calibre, in penetrating the tissues tears them, and occasions thereby severe pain."

In a second paper published on 28 August 1858, Wood (1858a) gives the following description of the syringe which subsequently became known by his name:

> The instrument is of the simplest construction, and is a modification of Mr. Ferguson's already alluded to. It consists of a small glass syringe graduated like a drop measure, and to this is attached a small needle, hollow, and having an aperture near to the point like the sting of a wasp.

From this description, it is clear that Wood's modifications of the Ferguson syringe consisted of improvements to the cutting extremity of the needle and the addition of graduation to the barrel. His reference to a "small needle" may perhaps indicate that he reduced the bore of the needle, although this is not specifically mentioned. In the following week (4 September), Wood (1858b) published a letter in which he states, in answer to a number of inquiries, that "the instrument" may be purchased from Mr. Young of Edinburgh, and refers his inquirers to an advertisement in the same journal (Fig. 6).

In his paper of 1858, Wood is able to say "In Edinburgh, I may mention, the use of this instrument has become nearly universal . . . ."

It is remarkable that no attention appears to have been paid to Wood's first (1855) paper, either in England or on the Continent. As will be seen later, it was his second paper, published 3 years later in the *British Medical Journal,* that resulted in the extension of the method beyond the borders of Scotland.

Comparison of the 1855 and 1858 papers shows the development of a desire to emphasize the originality of his technique. Whereas, in the earlier paper, Wood pays tribute to the influence of Valleix in directing his mind towards the invention, he is at some pains to disown his debt in the later paper. Valleix's method of blistering sensitive points along the course of the affected nerve was

> not nearly so successful as he led me to expect. I then varied the application very much. I raised the blister, removed the cuticle, and applied morphia (both in a liquid form and as a powder in ointments) on the surface, and found, contrary to the experience of Valleix, that the patient derived decided benefit from it.

Three years before, he had referred to the "immense improvement . . . effected in our treatment of neuralgic affections" by Valleix's observations, and by his "plan of treatment, which, as an external remedy I have largely employed ever since my attention was first directed to his work in 1842."

### 4. THE PRAVAZ LEGEND

This section is in the nature of a digression, which would be irrelevant were it not for the extraordinary persistence of the legend that

C.-G. Pravaz of Lyons was in some way responsible for the introduction of hypodermic medication. Assertions to this effect vary from vague generalities to specific inaccuracies; some of them are reproduced in Section 10. In the interest of historical veracity, the Pravaz story will here be considered in some detail, although it forms no essential part of the main theme of this paper.

On 10 January 1853, the year of his death, Pravaz (1853a) reported experiments upon the production of coagulation of the blood in large arteries, and suggested the application of his technique to the cure of aneurysm in human subjects. After experiments over a number of years on different methods of effecting coagulation, he conceived the idea of injecting iron perchloride of "maximum concentration" into the lumen of the artery. Pravaz was later invited by M. Marjolin, secretary of the Société de Chirurgie de Paris, to give a fuller account of his therapeutic invention. This he does in a letter (1853b) dated 17 May 1853, in which he recounts the steps which led up to the idea of injecting iron perchloride intra-arterially.

First, he tells of his earlier attempts at the use of galvanism as a physical coagulant, and then he proceeds to answer the question:

> Pourquoi, après avoir usé des moyens physiques, j'ai préféré l'emploi des moyens chimiques.

It is quite clear from Pravaz's own account that the only aspect of his new treatment which he regards as in any way original is the principle of the *use of a chemical coagulant.* The means employed to introduce the coagulant into the lumen of the vessel is discussed as an incidental. (It should be remembered that intravascular injections had first been practiced nearly two hundred years before.)

> Je pensait aussitôt à réaliser cette idée au moyen du trocart explorateur que l'on employie pour reconnaître la nature des liquides contenus dans certaines tumeurs.

These exploring trocars were, however, not sufficiently fine for the purpose, and Pravaz decided to have a finer trocar-and-cannula made for him.

> J'attendis l'occasion d'un voyage à Paris pour en faire exécuter de plus appropriés au but que je me propose. M. Charrière remplit parfaitement mes vues à cet égard. Je me procurai en outre une petite seringue dont le piston était manoeuvré au moyen d'un pas de vis . . . .

Pravaz's communication is followed by another of almost equal

length from a pharmacist, M. Burin de Buisson, on "la préparation du perchlorure de fer liquide considéré comme agent coagulateur du sang."

In his original paper, Pravaz describes the fine "trois-quarts"[4] as made of gold or platinum. The syringe was of silver (Gaujot, 1867). Pravaz's experiments were made on the carotid arteries of a sheep and two horses. The trocar-and-cannula was introduced "très-obliquement" through the vessel-wall by a gimlet action (*mouvement de vrille*), the trocar was withdrawn, the syringe was screwed on to the extremity of the cannula, and the injection was made by rotating the handle of the screw piston-rod. The purpose of the screw piston was to insure accurate measurement of small volumes. In all his animal experiments, Pravaz succeeded in obtaining a hard clot which obliterated the artery. He did not himself undertake any experiments on human subjects with aneurysm, but several surgeons, including Velpeau,[5] Pétrequin,[5] and Malgaigne, adopted the method. After a few successes came disaster, and in a memoir (1874) Malgaigne reported, of 11 cases treated, 4 deaths, 5 failures, and only 2 cures.

At no time did Pravaz practice hypodermic injection, nor did he claim directly or by implication to have "invented" a particular type of syringe. The question of the "invention of the hypodermic syringe" will be discussed in more detail later (see Section 10).

## 5. The Extension of Hypodermic Medication and the Development of the Syringe

Although subcutaneous morphine injection was widely practiced in Edinburgh as a result of Wood's first publication in 1855, it was "little, if at all known or used" in London (Hunter, 1865a). In 1858, as a result of Wood's second paper in the *British Medical Journal,* a young London surgeon, Charles Hunter,[6] adopted the method, and the subcutaneous injection, on varied theoretical grounds, of solutions of drugs aroused a wide interest that was to result in the general adoption of the technique in England, Europe, and the United States. The extension of the method on the Continent resulted principally from a long memoir read to the Paris Académie de Médecine by Béhier (1859). Béhier refers to "les

[4] In the French literature of the time, *trois-quarts* or *trocart* signifies the complete instrument, consisting of *canule* and *poinçon.*

[5] According to an account in the *Association Medical Journal* (1853).

[6] Hunter's contribution is fully discussed in Section 6.

tentatives des expérimentateurs anglais" which, he says, have not sufficed to make the technique a general one because of the inadequacies of the Fergusson [*sic*] syringe, which does not permit the injection of accurately-measured quantities of solution. (It will be recalled that Wood, by 1858, had graduated and in other ways improved upon the Ferguson syringe.)

It was Béhier who, for the first time, employed the "Pravaz syringe" (Fig. 7) for subcutaneous medication, Pravaz having been dead for six years. Durand (1924) has published a photograph of a syringe alleged to be the one originally used by Pravaz. This differs from the syringe in Fig. 7, in that the handle of the piston-rod is not flanged but resembles more the handle of a modern syringe. English and French surgical-instrument catalogues after 1860 contain figures of "Pravaz syringes," but these bear no resemblance to the instrument reproduced by Béhier.

The "Pravaz syringe," or, more accurately, *l'appareil Pravaz,* was a reduced-scale exploring trocar-and-cannula, to which a small metal syringe could be screwed after removal of the trocar. The syringe had a screw-piston, which descended on rotation of the piston-rod. This apparatus, recommended by Pravaz in 1853 for the injection of iron perchloride into aneurysms, was unsatisfactory because of the clotting of blood within the lumen of the cannula, where blood and coagulant mingled after the trocar had been withdrawn and the syringe was screwed on. Lenoir, in the same year, evolved two improvements, which are described by his interne, Masson (1853). Lenoir made a trial of Pravaz's coagulation treatment at the Hôpital Necker, Paris, in a patient with a popliteal aneurysm. Difficulty was experienced at the first two attempts, because of clotting of blood in the cannula. At the third attempt, Lenoir employed a glass-barrelled syringe made by Charrière "afin qu'on puisse voir si le liquide ne passe pas au-dessus du piston." He also used a second and finer cannula which was screwed in readiness on to the syringe, and was then passed through the lumen of the original cannula after withdrawal of the trocar. The cannula attached to the syringe could be filled with the coagulant fluid before passing it through the original cannula, and by this means the injection of air and the mingling of fluids in the lumen of the cannula were both avoided. Béhier preferred this complex equipment to the Ferguson syringe which, involving the principle of skin-puncture with a hollow needle *with syringe attached,* was the prototype of the modern hypodermic syringe. The hollow needle of the Ferguson

syringe was, however, "trois fois plus grosse" than the fine cannula of the Pravaz-Lenoir-Béhier instrument.

Béhier advises that either of two techniques of injection may be employed: *a.* insertion of trocar-and-cannula through the skin, removal of trocar, attachment of syringe to cannula, and injection; or *b.* insertion as above, withdrawal of trocar, and insertion through original cannula of finer cannula attached to syringe. The second technique has the advantage, says Béhier, that air is not introduced into the tissues. However, he usually employed the simpler method, and although able to elicit crepitus from the air in the subcutaneous tissues, he had not observed "le moindre accident local."

With both techniques, the procedure was concluded as follows:

> Une fois la seringue ainsi vissé sur la canule, on tourne le piston. . . . Chaque quart de tour du piston, que est à vis, donne issue par l'extrémité de la canule à une goutte de liquide médicamenteux.

Béhier points out the cubic capacity of the cannula should be estimated.[7] The smaller cannula has a capacity of 4 drops, and in order to deliver a dose of 1 drop into the tissues, 5 *quarter-turns* are therefore required. When injecting, there should be a pause after each quarter-turn, as rapid rotation produces drops of larger size and dosage become irregular. Of a total of 61 cases treated, 53 were of sciatica, neuralgia, or rheumatic pains, and these all received injections of atropine sulphate, in the employment of which Béhier was following Benjamin Bell of Edinburgh.[8] The remaining cases were of paralysis (7) and lead colic (1) which were treated respectively by injections of strychnine sulphate and morphine hydrochloride.

In all cases, affirms Béhier, a good effect was "constant et très-marqué." Of 18 cases of sciatica, "nous avons constaté positivement 12 guérisons," and in the remaining 6 a cure was "plus que probable," but the patients after announcing themselves cured did not return, and results could not be positively confirmed. When one considers the complexity of Béhier's technique of injection, which cannot have been other than a slow and disagreeable procedure for the patient,[9] it is perhaps not surprising that a third of these patients defaulted.

[7] Except, presumably, in such cases where the second technique is employed and the cannula is charged with solution beforehand.

[8] See Section 7.

[9] Hunter (1865a) refers to a still more disagreeable method used for the injection of quinine: "I must protest once more against the way all those gentlemen introduce the quinine —namely, with a common syringe after incising the skin with a common *lancet!*"

According to Erlenmeyer (1887), a Dr. Bertrand, "Badearzt in Schlangenbad," was responsible for the first application in Germany, early in 1856, of Wood's technique. Bertrand's first patient was a woman who had been treated by Wood in Edinburgh. Erlenmeyer says:

> Es handelte sich um die an Neuralgien und hysterischen Krämpfen leidende Frau eines englischen Arztes, bei der Dr. Wood in Edinburgh subcutane Injektionen von Morphium mit Erfolg ausgeführte hatte, und bei der später Bertrand dieses Verfahren, in dessen Anwendung er von Wood unterrichtet worden war, fortsetzte.

Referring to the United States, Kane (1880) writes:

> The first to use the hypodermic syringe proper in this country was Dr. Fordyce Barker, of this city [New York], who received one from Prof. Simpson, while he was visiting in Edinburgh, and used it here in May, 1856, on his return.

These, however, were isolated trials, and the general diffusion of the technique awaited the publication of papers by Wood, Hunter, and Béhier from 1858 onwards.

The elaborate Pravaz-Lenoir equipment used by Béhier was manufactured by the well-known Parisian instrument-maker, Charrière, but did not long survive and was replaced by the *canule-trocart*—or hollow needle—which was inserted through the skin with syringe attached. Charrière ultimately adopted this principle, retaining the screw-piston. Another Parisian instrument-maker, Mathieu, devised a new model, known as the *seringue décimale hypodermique,* of which the distinguishing feature was a bayonet-catch which enabled either a screw or a push-pull action to be employed at choice (Fig. 8). The thread of the piston-rod and the bore of the syringe were such that, with the bayonet-catch adjusted for screw-action, a half-turn delivered 0.05 g. of solution.

The third of the great trio of contemporary Parisian instrument-makers, Luer, discarded entirely the screw-action, and also dispensed with the screw-fitting, which joined needle and syringe, substituting the simple conical nozzle which has remained to this day (Fig. 9).

The only relic of the screw-piston was a runner which moved up and down on the threaded piston-rod. Luer's syringe was the immediate forerunner of the modern hypodermic syringe, although its essential distinguishing features—attached needle and push-pull action—were already present in the Ferguson syringe. The graduation, which Wood added to the barrel of the Ferguson syringe, is on the piston-rod of the Luer model. A different principle was involved in the Bourguignon (1860) syringe, which was made by Mathieu and was shown to the Academy

of Medicine in 1860. This resembled a glass pipette with a needle attached to one end and a rubber bulb to the other, pressure on the bulb delivering the fluid via the needle.

Reference has been made above to the alleged costliness and delicacy of the early syringes. Eulenburg (1880) credits Leiter,[10] a Viennese instrument-maker, with having contributed more than anyone else to the general use of hypodermic syringes by his substitution of vulcanite for some of the metal parts, including the piston (Fig. 10). This led to the marketing of cheaper syringes which, however, developed faults, and which Eulenburg describes as "cheap and poor" (*billig und schlecht*). Leiter's first syringe was devised in 1862. Two years later, he admits that this model is unsatisfactory owing to shrinkage of the vulcanite piston, and at the same time publishes a description with figures of a "simplified" syringe, in which a leather washer has been added to the piston and the piston-rod has been made hollow to accommodate the needle when not in use. Leiter observes that there can be few medical discoveries which have won such general acceptance among the medical public as that of subcutaneous injection, and adds:

> Die subcutane Injektionsspritze befindet sich jetzt schon nicht nur in den Händen der meisten Aerzte, sondern selbst in denen der Patienten . . . .

The Luer and Leiter syringes, which were essentially the same except for the materials from which they were constructed, remained the standard pattern until the introduction of the Record syringe, by the Berlin instrument manufacturer Dewitt & Herz.[11] This was a metal-and-glass syringe in which were incorporated a number of minor technical improvements. The piston was ground in to the barrel by precision work, and fitted with a patent concentric slip-ring. Piston and barrel fitted tightly without lubricant. The metal mounts were soldered to the glass barrel by a special alloy. The proximal cap could be detached and replaced by a simple bayonet-catch. There were no screws, and when dismantled the syringe comprised only 3 separate parts. The distal extremity was so constructed as to enable the barrel to be emptied to the last drop.

The Record syringe has survived to the present day substantially unchanged, but recent investigations (Medical Research Council, 1945)

[10] Leiter, welcher durch Einführung des billigen Hartkautschuk-materials vielleicht am allermeisten zur Verallgemeinerung der Methode beitrug.

[11] I have been unable to identify the date of introduction of the Record syringe. The description of its distinguishing features is taken from a Dewitt & Herz catalogue published in 1909. Schwidetzky (1944) gives its date of introduction as 1906.

Fig. 1. Lafargue's "Inoculation Medicamenteuse" (Bruns, 1869).

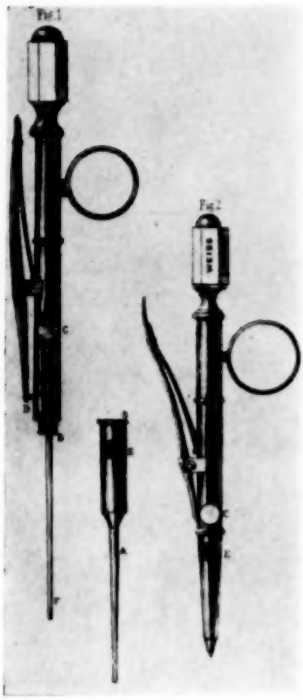


Fig. 2 (left). Von Bruns' Implantation Needle for the Subcutaneous Introduction of Pellets (Bruns, 1869).

Fig. 3 (right). Rynd's Retractible Trocar (Rynd, 1861).

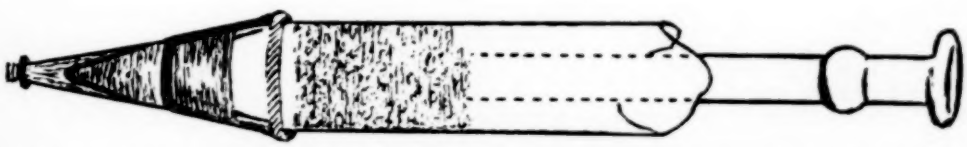


Fig. 4. Syringe used by Alexander Wood (Royal College of Surgeons, Edinburgh. Drawing by Mr. J. H. T. Smith).

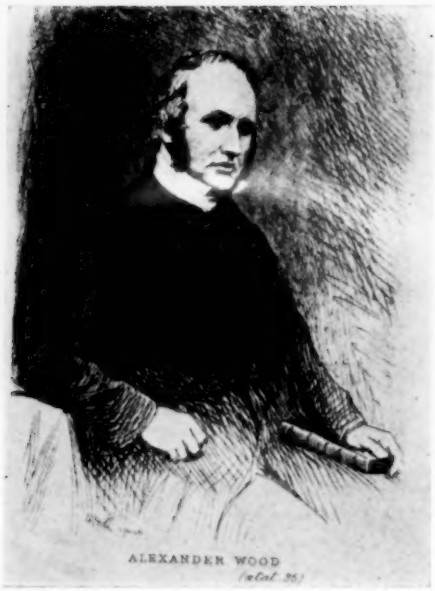



Fig. e. Alexander Wood, the Father of Hypodermic Injection.

**Dr. Alexander Wood's Narcotic**
INJECTION SYRINGES are manufactured by ARCHIBALD YOUNG, Queen's Cutler, 79, PRINCE STREET, EDINBURGH.
A. Y. Imports OBERHAUSER'S MICROSCOPES, the BEST and CHEAPEST Instrument for SURGICAL purposes.
Orders for these Instruments will be promptly attended to.

Fig. 6 (above). Advertisement for Wood's Syringe (*Brit. Med. Jour.*, 23 October 1858).

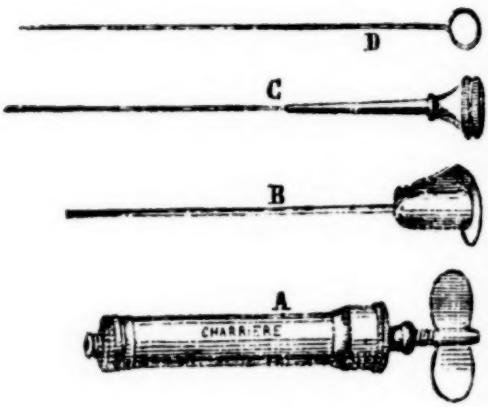



Fig. 7. (right). "L'Appareil Pravaz" (Béhier, 1859).

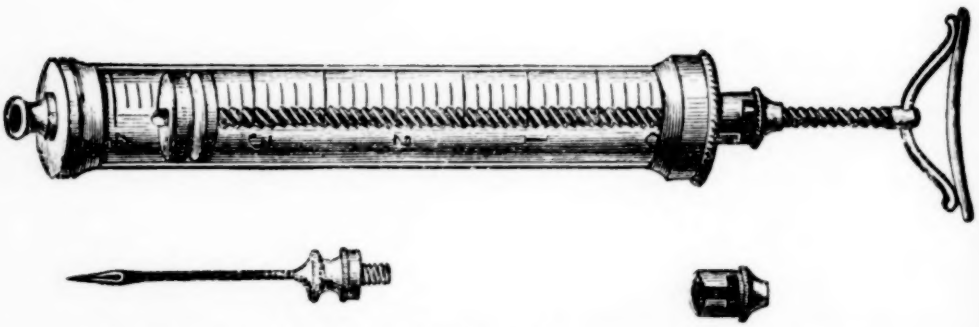


Fig. 8. Mathieu's "Seringue Decimale Hypodermique" (Bruns, 1869).

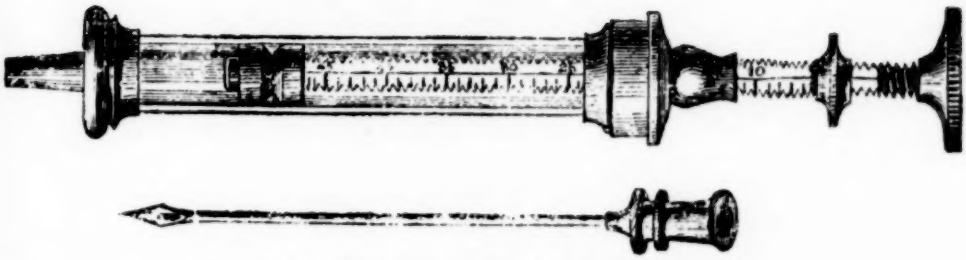


Fig. 9. The Luer Syringe (Bruns, 1869).

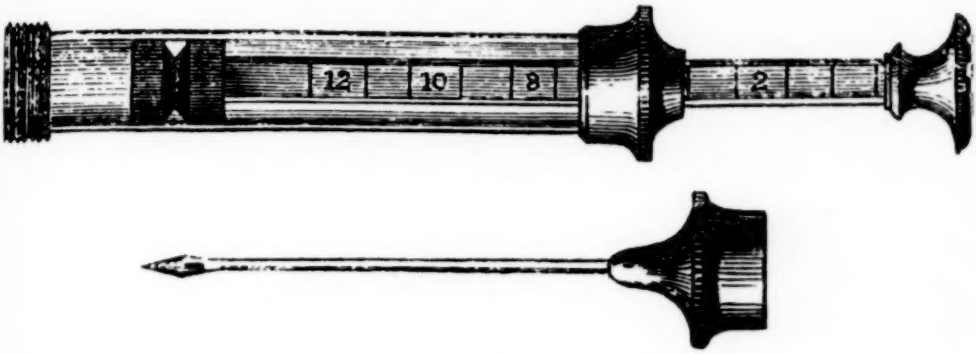
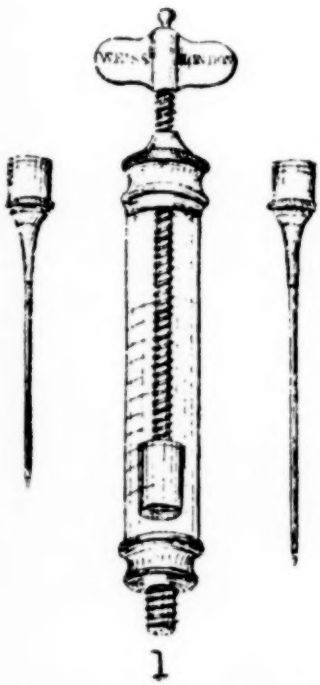


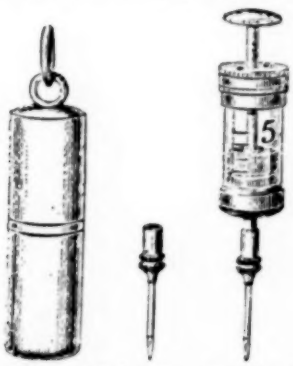

Fig. 10. The Leiter Syringe (Bruns, 1869).



Article 2787.

## A NOVEL SUBCUTANEOUS SYRINGE.

Messrs. Joseph Wood & Co., of York, are making an extremely neat and useful novelty in the shape of a watch-chain charm containing a subcutaneous syringe. The annexed diagram is of the actual size of the instrument. The syringe is graduated to




5 minims, and fits with its two needles into a neat gilt case, with double swivel ring to permit its being attached to the watch-chain. When in this position, it is ready for any emergency, and is not likely to be forgotten. Its moderate price should prove a recommendation.

Fig. 11 (left). Charles Hunter's Syringe (Weiss, 1863).

Fig. 12 (right). The Watch-Chain Syringe (*London Medical Record*, 1884).

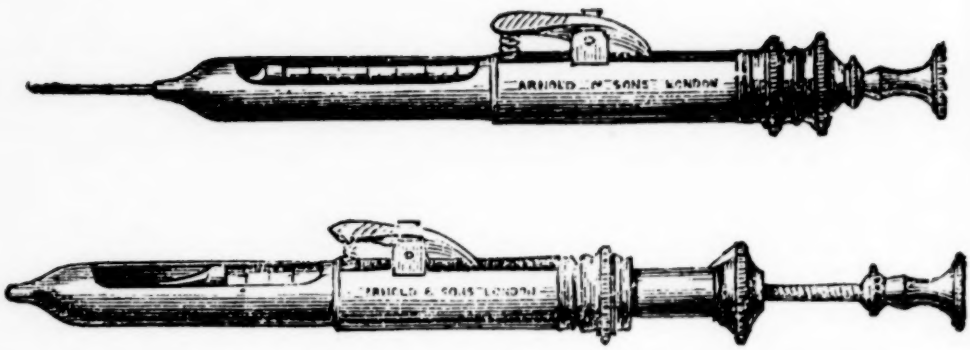


FIG. 13. "Arnold and Sons Improved Self-Acting Syringe" (Arnold, 1876).

FIG. 14. The Operation of Hypodermic Injection (Bruns, 1869). The syringe shown in use is the Luer model (see Fig. 9).

FIG. 15. The Last Hours of a Morphinist.

have shown that the difficulty of sterilizing these syringes should lead to their abandonment in favor of an all-glass type.[12]

The main lines of development of the "hypodermic" syringe have been sketched above. There were, however, many offshoots which had no survival value. In fact, the mere enumeration of minor variants named after their designers would occupy considerable space, and only a few selected examples will be given here. It may be mentioned parenthetically that Hunter took the retrograde step of adding a screw-piston to the Ferguson-Wood syringe (Fig. 11).

Several variants were introduced on the ground of greater convenience. In this category was the watchchain syringe *(London Medical Record,* 1884), (Fig. 12). An automatic syringe invented by Leiter (1875) was described in the *British Medical Journal.* After the publication of this description, Arnold & Son, the instrument manufacturers, wrote (1875) to point out that they had "for some time past" marketed a similar apparatus under the description "Arnold's self-acting hypodermic syringe." Release of the spring resulted in automatic puncture and injection of the fluid (Fig. 13). An apparatus for multiple injections was designed by a French medical student, G. Bay (1879). This consisted of a flask and pumping device, with valves which determined the direction of the flow.

## 6. "The Localisation of Wood" *versus* "The Hypodermical Treatment of Disease"

In 1836, Lafargue had clearly recognized that morphine paste introduced through the skin exercised a systemic action, but so pre-occupied was he with the supposed local effect that he did not appreciate the significance of his observation. In 1845, Rynd described his treatment of neuralgia, which was intended as a direct application of morphine solution to the affected nerve. He makes no mention of general effects and his later (1861) reference to the use of two instruments—a short one for superficial nerves, and a longer one for deep nerves—leaves no room for doubt as to his intention. Wood (1855) devised in 1853 the procedure of hypodermic injection as a means of "bringing some narcotic to bear more directly . . . on the affected nerve." As has been

[12] Schwidetzky (1944), gives "about 1896" as the date of introduction of the all-glass syringe.

described earlier, a conspicuous result of his first trial of the hypodermic injection of morphine was to cause profound somnolence.

Wood devotes nearly half of his first paper to a discussion of the "modus operandi" of his procedure. This discussion shows him to be no mere empiric, but an experimenter who was thoroughly versed in the literature of his subject. He divides the actions of medicines into "local or topical" and "remote," and, citing the observations and views of Brodie, Magendie, Christison, and other writers, discusses the mechanism of "remote" effects with particular reference to the rival theories of "sympathy" (action via the nervous system) and "absorption" (action via the bloodstream). Wood concludes that the evidence is in favor of "absorption," and that "the cellular tissue has a great power of absorption." In the treatment of neuralgia, he continues,

> it must be an immense advantage to secure—1st, A local effect, applied *directly* to the affected nerve. 2nd, A remote effect, ensuing almost *instantaneously* on the application of the remedial agent.

Summing up "the advantages which this new method of treatment seem[s] to offer," he concludes:

> That narcotics injected into the neighbourhood of the painful point of a nerve affected with neuralgia, will diminish the sensibility of that nerve, and in proportion diminish or remove pain.
>
> That the effects of narcotics so applied are not confined to their local action, but that they reach the brain through the venous circulation, and there produce their remote effects.
>
> That . . . cellular tissue . . . seems to offer an excellent surface for the absorption of the venous system.

From the foregoing, it would appear that Wood was fully appreciative of the potentialities of his new technique as a method of systematic medication, yet other considerations make it clear that his obsession with preconceived ideas of a local action on nerves led him, as had been the case with his predecessor Lafargue, to a false interpretation of his results. The title of his paper, "A new method of treating neuralgia by the direct application of opiates to the painful points," and his assertion that the method is "quite useless" in cases "where the disease has a central, not a centripetal origin" sufficiently illustrate this contention. But if further evidence on this point is necessary, it is to be found in his 1858 paper—after he had been practicing the method for 5 years—in which he relates the following history:

A lady, troubled with neuralgic pains, had been punctured upwards of one hundred times, always in different places; but no sooner had the pain been driven from one spot, than it took up its seat in another. At last, I had expelled it from every part of the body, except a corner of the head, and there I was puzzled how to deal with it. The fact was, I could detect no painful point in the scalp.

Wood then emphasizes that the injection must be made, *not* at the site where the patient complains of pain, but at the point which is tender on pressure. As Wood could find no such point on this occasion, he withheld the "puncture," and the lady was taken for several months to "the German baths" by her husband. No improvement resulted from this change of scene, and her husband brought her to see Wood again. Wood made two examinations and on the occasion of the second found "the point where the needle should be inserted." And, he concludes, "from that day she has never had a touch of neuralgia again, though she has suffered from rheumatic gout."

Although he refers later to the "truly astonishing" rapidity with which systemic effects are seen, and postulates a dual action from a combination of local and general effects, the case-history related above shows that, in practice, he discounted the systemic effects, and did not regard them as justifying the employment of the method in the absence of a "painful point."

On 16 October 1858, a few weeks after the publication of Wood's second paper, Charles Hunter, a house surgeon at St. George's Hospital, London, published a letter (1858a) in which he described the employment of "the local treatment, by narcotic injection into the part" in two cases. The first, a man of 55 with tic douloureux, received a course of injections of morphine from which he "obtained considerable ease," but these had to be discontinued because of the development of an abscess. The second patient was a girl of 18 who had had one eye removed and was suffering considerable pain in the remaining eye. In her case, morphine was "injected under chloroform into the eyelid." Further injections promoted sleep and considerable relief, but later an abscess necessitated discontinuance of the treatment. Hunter concludes:

[The narcotic] appears to exert more benefit on the local affection when it has to be absorbed from the part affected itself, probably from being brought more directly into contact with the nerves involved in the disease.

In a second letter (1858b) two weeks later, however, he writes:

I have given up the localization of the remedy as proposed by Dr. Wood, viz. the injecting of the narcotic into the most painful spot affected by neuralgia . . .

and have adopted the employment of narcotic injections into the cellular tissue of various parts of the body.

In the first case described in his previous letter, the development of an abscess had prevented continuance of the treatment, and morphine had instead been given by mouth—with no relief.

I once more commenced narcotic injections of the cellular tissue, not this time of the part affected with tic, but of healthy parts, such as the neck, arm, etc., and taking care that the same spot should never twice be the subject of injection. Since beginning this plan, the man has again received remarkable benefit.

In the case of the second patient also, local injections were discontinued and "various places in the integument of the arms" were chosen instead. Hunter was quick to see the significance of an observation made as the result of the accidental circumstance of sepsis, and he formulated the following conclusions:

1. That by the injection of the narcotic into the cellular tissue of a part distant from that affected with the neuralgia, the relief that follows appears quite as great as when the injection is into the cellular tissue of the neuralgic part.

2. That therefore the idea that the relief results from the localization of the remedy in the painful part is erroneous—equal relief being afforded in either case (injected into the painful part or elsewhere).

Having accidentally made an important discovery, Hunter succumbed to two temptations: he attempted to build a therapeutic system, and at the same time he conceived an exaggerated idea of his own originality. The title of a paper published in 1863, "Practical remarks on the hypodermical treatment of disease," illustrates the former point. Hunter introduced the term "hypodermic" to distinguish his "plan" from that of Wood. A few years later, he refers (1865a) to "the treatment of diseases, more especially of the central nervous system, by means of the Hypodermic Method. . . ." This term, he says,

was the name I gave this plan of introducing medicines in 1859, to imply treatment carried on under, or *beneath,* the true skin.

In a paper (Hunter, 1865b) read on 23 May of the same year to the Royal Medical and Chirurgical Society, the distinction had been more clearly made, as is shown by the following report in the *Medical Times & Gazette:*

It is now six years since Mr. Hunter proposed the injection of medicines into the cellular tissue with their general therapeutic effect in view. In the case of med-

icines thus injected for general effects, he called the method the "hypodermic," to distinguish it from the endermic, and from the local injection of Wood. . . . The hypodermic differs from the "method of Wood." The latter plan has for its object the local treatment of a local affection.

For seven years Wood had maintained silence, and Hunter's claims had gone unopposed. This last, however, was too much for Wood, and he wrote (Wood, 1865a):

He [Hunter] is reported to have said "the hypodermic differs from the method of Wood." To this I reply it does so merely in name. I called my method "subcutaneous," borrowing from the Latin; Mr. Hunter, in appropriating it, called it hypodermic, a Greek compound having precisely the same meaning.

Wood further affirms that local injection is often more efficacious than general, and protests that credit is due to whoever "showed the plan of using the cellular tissues as a medium for the introduction of remedies into the system."

To this, Hunter (1865c) replied:

Does Dr. Wood desire to claim double priority, as to his own theory and practice, and as to one which he admits is a modification, and which is both theoretically and practically different to his plan?

My proposal of the injection for general purposes which I made in the journals six years ago can lay no claim as regards priority with a method originated twenty-three years ago by Dr. Wood.[18]

Wood (1865b) countered this by pointing out that he had described general effects in his paper of 1855. He asserted that his claims to priority were not affected by the particular explanation of results that he had put forward.

So that I did not rest my claim on my *theory* being the correct one.

The final letter of this acrimonious and undignified exchange was one from Hunter (1865d), in which he reiterates his claims and says:

But Dr. Wood now hankers after more credit. . . . It was I who first publicly advanced the view . . . that the localisation of Wood in neuralgic cases was unnecessary.

At this point, the editor closed the correspondence. Numerous letters had been received from other correspondents, but the editor published only two of them, both of which took Hunter's part in the dispute. The paper read in 1865 to the Royal Medical and Chirurgical Society,

[18] Hunter erroneously antedates Wood's discovery by 10 years. For a discussion of this fallacy, see Section 11.

the report of which goaded Wood to challenge Hunter's claims, was described (Hunter, 1865b) by the President as

> one of very great interest; so great, that the Council had appointed a Committee to investigate it.

The report of this committee was published two years later (Medical and Chirurgical Society, 1867). It was based on (i) original experiments upon man and animals, (ii) personal evidence of "experienced medical men," (iii) "records of facts and other communications received in answer to a series of questions inserted in medical journals." Numerous drugs were tested, but the many experiments performed to settle the disputed question of local action

> failed to obtain any evidence to show that the local predominate over the general effects; they [the committee] must therefore express their opinion, that though no symptom results from injection at the part affected, which is not shared equally by injections at any other part of the body, yet practically it may be advantageous to localise the injection for the sake of those effects upon the mind which localisation will sometimes produce.

Summing up, the committee concludes, wholly in favor of Hunter's contention,

> That no difference has been observed in the effects of a drug subcutaneously injected, whether it be introduced near to, or at a distance from the part affected.

In spite of the unequivocal findings of the committee, belief in the "localisation of Wood" lingered on for many years. Jousset (1865), however, recognized the correctness of Hunter's view:

> Cette méthode, employée d'abord à titre de médication locale, n'a pas tardé à prendre en médecine une place plus importante. . . .
>
> Comme on le voit . . . la méthode hypodermique n'est autre chose qu'une mode d'absorption.

Béhier (1859) had refuted Hunter's contention that no advantage was gained by injection into the painful area, although he recognized the rapidity with which systemic absorption occurred, and advised that this route should be used for "agents très-actifs."

Anstie in 1868 favors Hunter's view, but with some reservations.

Bruns in 1869 categorically expresses his belief in the reality of a local action, although he admits that this is subordinate to the systemic effect:

> Angesichts dieser Erfahrungen kann eine örtliche sedative Wirkung der hypodermatischen Injection narkotischer Mittel nicht mehr in Abrede gestellt werden,

wogegen ohne Widerspruch zugegeben werden muss, dass die allgemeine Wirkung in der Regel die weit überwiegende ist.

Eulenburg (1880) contributes a long and exceptionally well-informed discussion of this problem. In an attempt to reach a satisfactory solution, he performed experiments upon healthy subjects, testing sensation in comparable injected and uninjected areas on both sides of the body. He affirmed from these experiments that there was a definite diminution in tactile discrimination in the injected areas. Control experiments with distilled water excluded a non-specific mechanical action, and Eulenburg concludes that there is a definite "local anesthetic" (localanästhesierende) action of both morphine and atropine on the nerves.

Es dürfte schon hieraus vorgehen, dass der örtliche sensibilitätsvermindernde Effect nicht etwa dem Orte der Einspritzung an sich, dem Einstich oder der Anfüllung des Unterhautzellgewebes mit Flüssigkeit zugeschrieben werden kann, sondern dass es sich dabei in der That um eine rein medicamentöse Localwirkung handelt.

In the same year, Kane (1880), in the United States, writes that:

Great diversity of opinion still exists amongst medical men as to whether an injection of morphine is as effective when made at a distance from the seat of pain, as when made directly over it.

In 1887, Currie writes:

The local action of morphine hypodermically injected, although it appears to be tolerably certain, is still a matter of controversy.

Even as late as 1895 an eminent pharmacologist, Carl Binz, is able to state, on clinical grounds, that it is advisable to

choose a point as near as possible to the affected part for the injection, especially if we know that by means of the lymphatics this portion of the skin is in direct communication with the affected part.

Enough has been said to show that the whole intention of subcutaneous medication, until Hunter's publication in 1858, was to produce *local anesthesia* by direct application of morphine or other alkaloid to the nerve or its immediate vicinity. In 1865, Eulenburg carried this intention to its logical conclusion by proposing morphine injection as a substitute for "time-consuming" chloroform inhalation, although he expresses some reserve as to whether a sufficient reduction of sensibility would be obtained in painful operations.

Es wurde dadurch in geeigneten Fällen ein zweckmässiges Surrogat für die nicht immer statthafte zeitraubende und ohne Assistenz nicht durchführbare An-

wendung der Chloroforminhalationen gegeben. Die Beobachtungen hierüber stehen jedoch noch sehr vereinzelt da, und es kann daher noch nicht mit Sicherheit entschieden werden, ob die durch Morphium-Injectionen bedingte Erregbarkeitsabnahme stark genug ist, um bei schmerzhaften Operationen dem gesteigerten Reiz das Gleichgewicht zu halten, und somit eine genügende locale Anästhesierung hervorzurufen.

In 1877 Foote, in the United States, described the excision from the arm of a colored woman, "without the slightest pain," of a "fibrous tumour weighing about one pound," into which 1/2 grain of morphine and 1/30 grain of atropine had previously been injected. In another case, a small wen was removed with similar happy results. Foote comments:

It had long been a favorite problem with me to discover a method of rendering painless the minor operations of surgery without employing chloroform or any anaesthetic inhalant.

It is noteworthy that it was not until 1884 that Koller first exploited clinically the local-anesthetic properties of cocaine. Lafargue, Rynd, Wood, and subsequent adherents of "localisation" had clearly conceived the possibility of local anesthesia, and thought that they had achieved it. Some of them carried this belief so far as to employ local infiltration of morphine and atropine as an anesthetic for cutting operations. In fact, it might be said of local anesthesia that the only thing lacking before Koller and Halsted was an agent that really worked!

The Reverend Thomas Brown, Wood's brother-in-law and biographer (Brown, 1886), says that his subject's fame will mainly rest upon

the discovery of the hypodermic method of administering drugs, whereby medicines are introduced in solution under the skin, and so pass directly into the circulation.

Brown depicts drug therapeutics as being almost in a state of chaos while the principal route was still via the stomach.

Such was the state of affairs when Dr. Alexander Wood approached the subject. . . . A certain line of reasoning had led Dr. Wood to the belief that benefit was to be expected from the injection of morphia under the skin.

This pious evasion hardly suggests the true story, that Wood was so obsessed by his desire to produce a direct local anesthetic action upon the nerves, that he failed to understand the import of his unintentional discovery (in which he had been preceded by Lafargue) of systemic medication by the subcutaneous route. Hunter, an empiric whose perceptions were not blunted by a priori assumptions, at once apprehended the significance of the effects of morphine injections remote from the

painful site. In retrospect, this may not seem to be a very remarkable observation, but the long survival of the belief in "localisation" shows that Hunter's claim to originality was not lacking in substance.

### 7. Drugs, Dosage, and Rationale

An account has already been given of the rival theories of local and general action of hypodermically-administered drugs. Although Hunter had in 1858 realized that the effects were not dependent upon the site of injection, and his findings had been confirmed by a scientific committee nine years later, the local theory persisted and had powerful advocates for many years. Reading these discussions in the light of modern knowledge, it is clear that many of the arguments put forward on both sides were fallacious and based upon faulty observations. In fact, the penetrating criticism made by Martin-Solon in 1836 is one of the few contributions to the subject which can be read with approbation today.

The modern reader is tempted at first to place some reliance upon the reports of therapeutic results achieved with morphine in various "neuralgic" conditions. But when he reads of identical results with other drugs which could not possibly have effected the results attributed to them, and even with pure water, his confidence is shaken. It becomes plain that many of the conclusions are based upon the therapeutic fallacy which is conspicuous in the practical medicine of all times—including the present—and which is neatly summarized by Paris (1843) in a chapter-heading to his *Pharmacologia*: THE ASSIGNING TO THE WORKINGS OF ART THE EFFECTS OF UNASSISTED NATURE: OR THE CONSEQUENCES OF INCIDENTAL CHANGES OF HABIT, DIET, &c.

The selection of drugs for use in various conditions had no rational basis, and the hypotheses devised to explain their action were based upon pure conjecture. The chemical achievement of the isolation of active principles of drugs, starting during the second decade of the nineteenth century, provided a new type of therapeutic agent which was applied indiscriminately by both old and new methods of administration. The principal drugs used emplastro-endermically by Lembert (1828) were morphine, strychnine, and quinine, but he also used a number of mineral and galenical preparations. Lafargue employed hypodermically, in addition to morphine, several opium preparations, atropine, strychnine, quinine, veratrine, aconitine, digitaline, hyoscyamus, colchicum, anti-

money, and croton oil, to which Martin-Solon added stramonium, henbane, and castor oil.

Rynd (1845) used morphine in his only two published cases. The heroic doses employed (10 grains in one case and 15 in the other) do not speak well for the efficacy of his technique, for neither patient appears to have developed alarming symptoms.

Wood (1855) says of neuralgia:

> In some cases, I have seen relief follow the application of an ointment containing strychnine to the blistered surface, but this must be used with great caution, as very disagreeable results often ensue from its use.

Later (1858) he speaks rather more candidly of his experiences with strychnine:

> One remarkable case, many years ago, I cured by applying nux vomica to the blistered surface; but I never tried it again, for it seemed likely to kill two people: my patient, an old lady, who nearly died of the poison; and myself, then a young doctor, who nearly died of the fright.

After his introduction of subcutaneous injection, however, Wood became more conservative and seems to have been content to confine himself to morphine-containing preparations.

Hunter (1858) for his first two cases used daily doses of $1^1/_3$ and $2^2/_3$ grains of morphine.

Early in the following year, he refers (1859) to the injection of colchicum and hyoscyamus for the treatment of neuralgia. In 1863 Hunter claims to have been the first to inject atropine subcutaneously:

> Hardly recognized as an internal remedy, and described as a poison of such great power, it was with no small anxiety that, in 1859, I first injected atropine beneath the human skin.

This claim is repeated two years later (Hunter, 1865a), but Hunter ignores the fact that Benjamin Bell (1858) of Edinburgh, after using Wood's "ingenious plan" for neuralgia, decided on 11 February 1858 to inject "ten minims of a solution of atropia" in a young woman with severe facial neuralgia, with the result, according to Bell, that the patient experienced immediate relief.

Although Bell refers only to one case, Béhier, when he adopted subcutaneous injection in the following year, would appear to have been much influenced by Bell's report, to which he refers briefly. All Béhier's neuralgic cases were treated with atropine injections. The dosages that

he employed were enormous—$1^1/_3$ to $2^2/_3$ mg. being typical single doses. It is hardly surprising that atropine intoxication was a conspicuous feature of the treatment, and Béhier dispassionately describes some of the symptoms towards the end of his paper. All patients complained of malaise, dizziness, dryness of the throat, and visual disturbances. Some had visual hallucinations—one woman saw winged rats. A man who had received an injection of $3^1/_3$ mg. ran deliriously about the hospital, but later settled down to a quiet night. On the next day, however, without further treatment, he again became delirious, and knelt on his bed imploring Béhier to save his wife and children. With him, as with others, "l'opium arrêta tous les phénomènes toxiques." The idea of the antagonism of atropine and opium was also derived from Bell.

Béhier tried the effects of high doses of morphine in two cases of neuralgia, but found the results "moins satisfaisants" than those attributed to atropine. He also claims to have been the first to inject strychnine subcutaneously, "ce que personne n'avait tenté antérieurement." None of these claims to priority of subcutaneous injection of various drugs amounts to very much, however, as Lafargue had already adminstered them subcutaneously by "inoculation" over 20 years before. Béhier's paper appears to have had the effect of putting atropine into a position at least of parity with morphine in the treatment of the doubtless miscellaneous group of conditions described as neuralgic.

As has already been mentioned, Hunter attempted to build what, by his own contention, was merely a mode of administration, into a therapeutic system, which he described (1863) as "the hypodermical treatment of disease." This was to him an aetiotropic, not a mere symptomatic treatment.

> Injection of any part of the cellular tissue (provided the alkaloid employed be the correct one) will remove neuralgia, and a morbid state of the nerves in any other part of the body. (Hunter, 1865a.)

He claimed (1865a) to be the first to have shown that "centric affections, such as mania and delirium . . . could be curatively treated by subcutaneous injection."

Morphine, according to Hunter (1865a) may be an effective palliative in sciatica "but it is not always the agent to strike at the root of *this* disease."

> Atrophia [*sic*] and strychnine, I believe, strike more surely at the root of the

sciatica in many cases, than does morphia or any opium alkaloid. They produce a more tonic change in the affected nerve.

"The hypodermic treatment" (see Fig. 14) may also be used "to diminish vascular action and inflammation," and in "blood diseases" the method possesses a "superiority . . . over others in checking disease." In nervous affections, "atropine is the goad that arouses the torpid nerve, and so gives power, as well as ease from pain."

Hunter (1865a) devotes an entire chapter of his book to "Morphia as a tonic in certain conditions of the nervous system."

> As a nervine tonic, I propose morphia by this plan. . . . I have frequently used this alkaloid, not as an anodyne, not as an hypnotic, not for the relief of spasm, but to remove cerebral nervous debility. . . . In such cases I hold that morphia does produce a tonic effect upon the nervous system, and when given by the cellular tissue, it is more essentially tonic than when given by the stomach.

It may well be that Hunter had made morphine-addicts of his patients, and that morphine injections did indeed appear to have a "tonic" effect upon them.

Anstie (1868) quotes Trousseau as saying that both morphine and atropine are "less active by the skin than by the stomach" and that "1/10th or even 1/5th of a grain may be used without causing any inconvenience." Anstie describes this assertion as a "most astounding blunder" and points out that doses as small as 1/60th grain can cause "serious symptoms."

Most of the therapeutic results claimed by the early users of hypodermic medication are so inconsistent with our present knowledge of the pharmacological actions of the drugs administered, that little reliance can be placed upon any of the observations made. This scepticism is fully justified—if further justification were necessary—by the astonishing claims made for the injection of pure water as a therapeutic procedure. On 20 August 1875, a Dr. Lafitte read a paper (1875) on the therapeutic effects of pure or distilled water. Three years previously in Paris he had seen Dieulafoy, whose interne he was, "guérir ou soulager d'une façon miraculeuse et immédiate" a boy with acute articular rheumatism by means of several subcutaneous injections of ordinary water. Lafitte then recounts his own experiences in the use of this treatment, which he started to employ in 1872. Always, he says, "le succès est venu sanctionner mes tentatives." Discussing the modus operandi, Lafitte concedes that there may be a psychological effect in some patients, but he

inclines more to the view that the injected water exerts a physical action[14] upon the cutaneous nerve-endings, and he concludes by suggesting that water should replace morphine by injection. Lafitte's paper was followed by letters from several writers who had previously been practicing water injection. One of them (Lelut, 1875) recounts how he had been led to adopt this treatment by the carelessness of a servant who had upset his bottle of morphine solution. In order to conceal the accident, she filled the bottle with water. Lelut unwittingly injected water in a patient with sciatica, who had previously been receiving morphine injections. On the following day, the patient thanked Lelut effusively, saying:

> Docteur, comme je vous suis reconaissant! Vous m'avez calmé aujourd'hui sans me donner mal au coeur.

This led Lelut to examine the bottle, when he found to his astonishment that it contained only water. Since this experience, he says, he has employed nothing but water in hypodermic injections. Further letters on "injections hydriques" followed, both from enthusiasts and from detractors.

Lelut's letter was fully reported in the *British Medical Journal* (1875) for 27 November, and this report was followed by a letter from Dr. Burney Yeo (1875) claiming that a Dr. G. W. Moore had substituted water for morphine and atropine injections with results that were "entirely successful."

> We did not, however, think of claiming for our expedient any other virtue than that of making a mental impression on our patient's nervous system. . . .

In the same issue is a leter from a Dr. Griffith, who claims to have used intramuscular injections of water since 1868 in the treatment of sciatica and lumbago. In the following years, many other papers on the therapeutic value of water by hypodermic injection were published.

A note on peculiar solvents may conclude this section. It has already been mentioned that Rynd dissolved morphine for subcutaneous administration in "one drachm of creosote." Wood, on the occasion of the first hypodermic injection in 1853, used "sherry wine" as a solvent for morphine. His reason (1858a) for doing so was that

> I thought it would not irritate and smart so much as alcohol, and . . . it would not rust the instrument as a water solution of opium would do.

[14] This is in contradiction to Eulenburg's findings (see Section 6).

## 8. The Belated Recognition of Addiction to Hypodermically-Administered Morphine

It is improbable that the cruder methods of endermic and hypodermic administration of morphine were ever practiced with sufficient regularity or frequency to produce addiction. With the coming of hypodermic injection, however, morphine was injected with reckless frequency, and apparently without a thought of the possibility of addiction. Three years after he had introduced hypodermic injection, Wood casually speaks of having given over 100 injections to a female patient with neuralgia. When hypodermic injection was universally adopted, physicians and patients alike unwittingly became slaves to morphine. Leiter (1864) describes patients as well as doctors as having their own hypodermic syringes. In an editorial comment on a publication by Prof. Nussbaum of Munich, in which he described the accidental injection of morphine into a vein, with disagreeable consequences, the *Medical Times & Gazette* writes in 1865:

> Suffering from neuralgia, he had injected morphia under his own skin more than 2000 times—sometimes to the extent of five grains of morphia in twenty-four hours.

The large number of injections, and the high dosage employed, are not considered worthy of comment, and there is no reference to addiction.

Hunter (1865a) recommended morphine injection as a "nerve tonic in cases of great nervous exhaustion, or of irritability or great mental depression."

The committee on hypodermic injection of the Royal Medical and Chirurgical Society reported in 1867 that:

> To confirmed opium-eaters this method has been found of much service, smaller doses than those previously taken by mouth being requisite.

The typical attitude of a patient to "the puncture" may be well imagined by reference to the words which Lafitte in 1875, illustrating only the popularity of morphine injections, says are heard on all sides:

> Docteur, docteur, vite une injection, je souffre comme une possédée, j'ai ma névralgie, etc.

It was from Thomas Clifford Allbutt that the first considered warning of the dangers of morphinism came in 1870:

> Among the numerous essays and records concerning the hypodermic use of morphia which have been published of late, both at home and abroad, I cannot call

to mind one in which its possible dangers have been considered; . . . while my fears were indefinite, I felt the time had not come for me to speak. Now my experience has been greater, I have a large number of cases before me, and yet the uncomfortable fear of mischief is growing rather than diminishing.

Is it not true, he asks,

that we are now often consulted by patients who have been injecting themselves daily or more than daily during long periods of time, for neuralgias which seem, nevertheless, as far from cure as they were at the outset?

Allbutt then refers to nine patients suffering from "various forms of neuralgia" in whom

the hypodermic use of morphia has been constantly practised for periods varying from nine months to three years. . . .

They seem as far from cure as they ever were, they all find relief in the incessant use of the syringe, and they all declare that without the syringe life would be insupportable.

In a postscript to his paper, Allbutt quotes a relevant passage from a German writer, Niemeyer, to which his attention had been drawn by Dr. Fothergill of Leeds, and which refers to the development of an irresistible need for injections, which is often described as a sort of "hangover" *(Katzenjammer)*.

Allbutt's point of view was, however, by no means popular, although there must have been an abundance of evidence to support the fears that he expressed. Doubtless many physicians did not care to admit their own addiction. Moreover, the very urgency of their patients' demands for morphine, added to the fact that oft-repeated injections must have been a substantial source of income, may have contributed to the tendency to rationalize or ignore the disagreeable truth. Oliver (1871) followed Allbutt with an admission that addiction occurred, but claimed rather unconvincingly that it was justifiable to "set up a morphia-habit, and then get safely over this habit by firmly withholding the morphia. . . ." A more question-begging solution can scarcely be imagined! Anstie (1871) followed Oliver by an even more unrealistic approach. He admits that

the subcutaneous injection of morphia has become a comparatively common household remedy among certain classes of society for some years past. More especially among the very numerous persons, chiefly women, who suffer either from neuralgia of greater or less severity, or even from attacks of nervous depression and sleeplessness without positive pain, it has become a too common practice to inject themselves, or to get injected by their servants, whenever they feel symptoms of their besetting trouble.

While disagreeing with self-injection, Anstie claims that small and "purely stimulant doses of morphia" are "perfectly legitimate" in elderly patients with chronic diseases, such as sciatica. He defines such doses as of 1/12th to 1/4 grain, with which there is "no vestige of *narcotic* action"!

As late as 1880, Eulenburg speaks blandly of having given over 1200 injections of morphine to a patient with neuralgic pains. In another context he refers to the increasing incidence of "Morphiumsucht," or morphiophilia, which, he says, has to a certain extent discredited morphine injection in the eyes both of the medical profession and the public. However, his admission of the existence of morphine addiction is a grudging one, and he is inclined to regard it as a consequence of the abuse of hypodermic injection by patients. He points out, in defence of the administration of morphine by injection, that there are extensive abuses of drugs when they are taken by the more usual method of oral ingestion. Apart from the obvious example of opiophagy, he says, it is necessary to recall only the considerable consumption of chloral in England and America, and the arsenic-eaters of Steiermark.

Enough has been said to show that the dangers inherent in morphine injection were not anticipated, and were for several years quite unrecognized. Even when morphine addiction had developed on a wide scale, the medical profession was extraordinarily slow to admit its significance, and sought refuge from unwelcome truths in sophistry and rationalizations.

### 9. Sepsis Following Hypodermic Medication

As the origin of hypodermic medication in its several forms was pre-Listerian, none of its early practitioners had particular reason for anticipating septic complications. Lafargue does not appear to have been troubled by abscess-formation—or if he was, he does not tell us about them. It seems probable that many of the stab-wounds made in the course of his "inoculation" of medicaments were followed by some degree of suppuration, but this would have been regarded as part of the normal process of healing. His technique of pellet-implantation was perhaps associated with more serious possibilities of infective complications, but if they occurred Lafargue preferred not to mention them.

Rynd says nothing of inflammatory sequelae, but he reported only his first two cases and gave only the scantiest details about them. Whether

he failed to develop his crude technique because it proved inefficacious or because of septic complications is a matter for conjecture.

With Wood's introduction of the subcutaneous injection of substantial volumes of fluid—doubtless often grossly contaminated—there was an obvious enhancement of the risk of infection. Wood, however, seems to have been favored with remarkable good fortune, for there is nothing in his writings that would suggest that any of his patients suffered from serious infections.

Charles Hunter was less fortunate, for he had to discontinue treatment of his first patient on the 24th day because of "a considerable-sized abscess which has been gradually forming in the last few days" (1858a). In his second case, injections into the region of the eye were followed by suppuration of such severity that 3 incisions were necessary to relieve it. It was the occurrence of these septic complications that induced Hunter to inject morphine at a site remote from the painful area, and thus led him in 1859 to the observation that remote injection was as efficacious as "the localization of Wood." He also made the further, and unwarrantable, deduction:

> That with the abolition of localisation of the remedy the great objection to narcotic injection is done away with, because no inflammation or abscess follows a single narcotic injection.

Béhier (1859), however, disagrees with Hunter on this point. Although he adheres to "localisation," he has not seen a single "accident local" in 227 injections. This he attributes to the use of the Pravaz cannula, instead of the coarser needle of the Ferguson-Wood syringe.

In 1865 Hunter returns to this problem, (1865a) "But is there no fear of abscess or diffuse inflammation following the puncture?" His answer is an emphatic negative:

> No, certainly not. For the last three years I have seen no local ill effect of any kind from the puncture. . . . As regards *single* punctures, much, I believe depends upon the care and the celerity with which the puncture is made. To bungle with the point of the instrument is to irritate, and an irritated puncture might inflame by friction. To employ a fluid otherwise than neutral would also irritate.

He adds a warning that abscess or inflammation may follow repetition of injection at the same site.

In the same year, Eulenburg (1865) characterizes the fear of local inflammation following injections as completely groundless, and supports his assertion by reference to a patient in whose breast over 1200 injections

were performed. Fifteen years later he expresses (1880) substantially the same view, although with some reduction of emphasis:

> Auch alle späteren Beobachtungen haben mir bestätigt, dass die blose Häufung der subcutanen Injectionen auf einem verhältnismässig kleinen Terrain an sich üble örtliche Folgen nicht zu involviren braucht. Wo solche dennoch auftreten, da wird man sie meiner Ueberzeugung nach fast ausschliesslich *dem reizenden Character des eingespritzen Medicaments, resp. der zur Injection dienenden Flüssigkeit* zuzuschreiben haben. . . . Nirgends wird man finden, dass Injection von reinem Wasser oder auch von einer klaren wässerigen oder glycerinigen, nicht angesäuerten, filtrirten Morphiumlösung Phlegmonen und Abscesse hervorgebracht habe.

Further on, Eulenburg refers to the possibility of transmission of infectious diseases, such as "Syphilis; Wundkrankheiten u.s.w.," which, he says, cannot be excluded, although the literature contains no direct confirmation of such an occurrence.

However, Kane, writing in the same year (1880), leaves the matter in no doubt. He had published a questionnaire in "every prominent medical journal of Great Britain, France, Germany, and America," and one of the questions included was: "Have you had inflammation or abscess at the point of puncture?"

Referring to morphinists, Kane says:

> In some of these persons the condition of the body is terrible. Abscesses are to be seen at every stage, from those just forming to those just healing or healed; large and small cicatrices, reddened, bluish, hardened or doughy lumps, ecchymoses; patches of gangrene of various sizes cover the body everywhere, "leaving" as some of my correspondents express it "hardly a spot of healthy skin large enough to be covered by a dime."

The condition is vividly illustrated in Fig. 15 and described by Kane (1880):

> The cut . . . represents one of the male nurses at Bellevue Hospital, who contracted the habit by stealing the house physician's syringe to give himself an occasional injection. The effect was so pleasing that he continued the practice and finally died. The wood cut here given represents the condition of his body a short time before death.

In most of the earlier writings on subcutaneous injection, there is a remarkable absence of references to septic complications, and modern experience with diabetics would seem to indicate that in many cases frequent subcutaneous injections can be made with impunity, although aseptic precautions may be absent or futile.

## 10. Some Retrospective Priority Claims in Regard to Hypodermic Injection

According to Kane (1880), two New York physicians, Taylor and Washington, published in 1870 a claim to have employed subcutaneous injection of morphine solution in 1839.

> They made an incision into the skin and then injected the drug into the subcutaneous cellular tissue with an Anel's syringe. . . . Drs. Taylor and Washington do not claim to be the first to use morphine in this way, their attention having been called to it by seeing an extract from some provincial French journal recommending it.

This suggests that Taylor and Washington may have read a reference to Lafargue's first (1836) inoculation technique, which, however, was very different from the technique that they adopted.

Sieveking writes in 1861:

> Dr. Kurzak, of Vienna, was, I believe, the first to employ the subcutaneous or hypodermic method, which was then largely used by Dr. Wood, of Edinburgh. . . .

Several later writers, including Kane (1880), refer to Kurzak as a precursor of Wood, but the present writer has not been able to trace any relevant publication by Kurzak, or, indeed, any earlier or more precise statement of Kurzak's alleged employment of subcutaneous medication than that of Sieveking.

Gaujot refers in 1867 to a method described by Langenbeck in 1856. Langenbeck is said to have introduced a lancet in several directions through the same skin-puncture.

> La communication de ces trajets, à orifice commun, forme une cavité unique, dans laquelle on peut faire pénétrer le liquide médicamenteux.

Gaujot does not describe how the fluid was introduced, and the present writer has been unable to refer to the journal in which Langenbeck published the description of his technique. However, Eulenburg (1880) says that Langenbeck claimed retrospectively to have anticipated Wood by injecting solutions of medicaments subcutaneously with a fine eye-syringe. Langenbeck abandoned this procedure in favor of his *Flächenimpfung* (see Section 2).

None of these claims, however, can be regarded as significantly affecting the history of hypodermic medication.

## 11. Some Persistent Errors in the History of Hypodermic Medication

*a. More about the Pravaz Legend.* Some space (section 4) has already been devoted to a discussion of Pravaz and the purpose and nature of his technique of intra-arterial injection. In the light of this discussion, it is perhaps of interest to refer to some of the statements connecting Pravaz with hypodermic medication. Durand (1924), in a biographical sketch of Pravaz, describes him as

> l'inventeur de cette chose mystérieuse et simple qu'est "l'aiguille creuse," discrète messagère du baume des grandes douleurs . . . .

As we have seen earlier, the hollow needle made its first appearance in the Ferguson Syringe, and was adopted later by Luer.[15]

Durand then describes the supposed Pravaz syringe as "le grand instrument de la médecine expérimentale et de la médecine curative." In a burst of hyperbole, he goes on to claim that the name of Pravaz will remain inseparably linked with that of Pasteur.

In 1927, the *Presse médicale* reported a campaign to save the memory of Pravaz from oblivion by the erection in his native town, Pont-de-Beauvoisin, of a monument to the "father of the hypodermic method" (père de la méthode hypodermique).

Other writers have conspired to perpetuate the fallacy that Pravaz fathered either the hypodermic administration of drugs or the syringe which later came to be applied to that end. Macht (1916), in a useful review of intravenous and subcutaneous medication, refers to "the instrument of Pravaz, which is the prototype of our modern syringe." Sudhoff (1922) writes:

> Alexander Wood in Edinburg . . . und Charles Gabriel Pravaz . . . in Paris . . . machten das hypodermatische Injektionsverfahren bekannt.

Cumston (1926) goes even further astray:

[15] In 1879, Claude Bernard made the claim: "C'est moi qui ai inventé la seringue piquante." In support of this claim, he cites an earlier volume of lectures, published in 1855. Reference to this volume shows that Bernard described in a lecture delivered on January 27, 1855 a syringe bearing a cannula with a sharpened extremity—that is, a hollow needle—"ce qui permet de percer directement la peau et de pousser l'injection en même temps." The published lecture includes an illustration of the syringe, with hollow needle attached. The instrument was used for the experimental injection of sugar solutions subcutaneously in rabbits. Bernard may conceivably have evolved this syringe earlier than 1853 (the date of the Ferguson syringe), but there is nothing in his original description of it to suggest that this was so, and, in any case, Bernard's instrument does not appear to have influenced the evolution of syringes used clinically for hypodermic medication.

In 1845 Rynd invented the subcutaneous method of introducing drugs into the organism, a method popularized by Pravaz. . . .

Garrison (1929) refers to "Pravaz's hypodermic syringe."
Comrie (1931) writes:

Alexander Wood . . . was the first person in Britain to use the hypodermic syringe, though priority of discovery cannot be claimed for him, since Pravaz had already used this form of medication in 1851, and published a description of his syringe in 1853.

Adams (1937) writes that the modern hypodermic syringe derives from the instrument of Pravaz, "who made many improvements and who published his results in 1853." Referring to hypodermic administration, Adams says that it became of universal prevalence "not many years after its popularization by Pravaz." Castiglioni (1941) says that Pravaz in 1853 "published his description of the first hypodermic syringe. . . ."

Schwidetzky (1944), in an article on "History of needles and syringes" which abounds in inaccuracies, says that:

With the invention of the pravaz needle by Dr. C. G. Pravaz . . . the basic principle of the hypodermic needle became established. This needle was made of steel, with hard rubber hub.

Keys (1945) attributes to Wood in 1853 the invention of "the modern type of metallic hollow needle," and adds that

in that same year . . . Pravaz attached an improved hollow needle to a specially constructed syringe.

Lundy (1946) says, "in 1853, Wood invented the hollow needle and Pravaz the hypodermic syringe."

The most remarkable and circumstantial account of Pravaz's supposed responsibility for hypodermic injection is that given by Fülöp-Miller (1938). Pravaz, according to this writer,

became principal of a home for the aged, where he had ample opportunity of watching the miseries that result from the maladies of advanced life. What was to be done for the relief of this suffering, for the alleviation of these miseries?

Morphine, says Fülöp-Miller, "was not well tolerated by the enfeebled stomachs of elderly persons." Pravaz left the home for the aged, but

was continually pondering the question which had begun to worry him when he was in charge of the home for the aged and had found the stomachs of the inmates intolerant of Sertürner's morphine. . . . How was it to be introduced *directly* into the blood stream instead of by way of the mouth and stomach.

As far as has been ascertained, Fülöp-Miller's account is completely fictitious.

In view of the number of assertions to the effect that Pravaz not only introduced hypodermic injection but popularized it, it is as well to emphasize:

i. That Pravaz died 2 years before the introduction of hypodermic injection in 1855.

ii. That Pravaz was concerned only with a form of intravascular sclerosing therapy by injection of iron perchloride into the lumen of an aneurysmal sac in order to produce a clot.

iii. That he performed experiments on the arteries of healthy animals, but treated no human patients. For this he used a fine trocar-and-cannula made to his order, and that he "ordered" a syringe for the purpose from a well-known Parisian instrument-maker.

It is also as well to recall

iv. That intravenous injection in animals and man had been practiced intermittently for nearly 200 years.

v. That syringes of metal and other materials had long been used for therapeutic and experimental purposes. In this connection, it is interesting to note that the 1843 catalogue of the London instrument-maker, Weiss and Son, contains a specification of a metal syringe with a "Weiss's Patent Metallic Piston" which is said to be "applicable to every description of Syringe, whether Philosophical or Surgical." Gaujot (1867), referring under the heading "Seringue de Pravaz" to hypodermic injection, points out that, in France, it was not necessary to invent new instruments for hypodermic injection.

> On prit simplement la petite seringue employée depuis longtemps dans les expériences physiologiques.

Pravaz, he adds, had proposed the use of this syringe for the injection of coagulants.

It is plain from the accounts of Pravaz himself that he regards his technique as possessing novelty only in that it involves coagulation of the blood by chemical means. There is no suggestion anywhere that the use of a syringe for this purpose is a matter of any originality.

However, it seems doubtful that Pravaz has a just claim even to have originated the intra-arterial use of chemical coagulants. Prof. D. Brainard of Rush Medical College published in the *Lancet* of 20 August 1853 a

claim to have anticipated him in this respect. Brainard had read in a French journal an account of Pravaz's experiments and of their clinical application by others in the treatment of aneurysm. He gives a circumstantial account of his own employment of iron lactate for a similar purpose—the treatment of an "erectile tumour of the orbit."

> I punctured the tumour at its most prominent part with the infiltrating canula . . . on withdrawing the stylet . . . a fluid drachm of the above-named solution [8 grains of iron lactate in 1 drachm of distilled water] was immediately thrown in with a small syringe, constructed for the purpose. . . .

This operation was performed on 14 December 1852, and a similar procedure had been performed intravenously in a human patient a year previously. Since 1850, Brainard had carried out experiments on dogs, in which he had successfully obliterated the veins.

In the 1874 edition of Malgaigne's *Manuel de médecine opératoire* the chapter on aneurysms contains a sketch of the development of the use of the injection of coagulants. According to Malgaigne,[16] the original idea belonged to Manteggia, and Wardrop in 1841 advised the injection of acetic acid with an Anel's eye-syringe in the treatment of aneurysm.

The question now arises: Who was the "inventor" of the hypodermic syringe? The answer is surely that there was no such invention. The invention lay in the adoption of the subcutaneous route by Wood for the administration of solutions of drugs. For this purpose he employed a syringe (Ferguson's) that was conveniently to hand, but had been devised for a different purpose. This makes nonsense of the suggestion that there was ever an "invention" of a "hypodermic syringe." Ferguson's syringe, inasmuch as it embodied the important principle of the hollow needle (as opposed to the trocar-and-cannula inserted as a separate operation), may be regarded as the precursor of the modern syringe used for hypodermic medication. Every characteristic feature of the so-called "Pravaz syringe" was ultimately abandoned. First, the metal barrel was changed to glass. Then the cumbersome trocar-and-cannula device was replaced by the hollow needle. Finally, the screw-piston fell into disuse.

How did the Pravaz legend originate? Probably because Béhier in 1859 used "l'appareil Pravaz" in preference to the Ferguson syringe, and

[16] No attempt has been made by the present writer to verify these statements of Malgaigne.

although the syringe was later modified until it changed beyond recognition, it still retained the name "seringue de Pravaz."

*b. The Date of Wood's First Subcutaneous Injection.* Many writers, including Keys (1945), Schwidetzky (1944), Terry & Pellens (1928), Bartholow (1891), and Kane (1880), give 1843 as the year in which Wood first performed subcutaneous injection. Hunter (1865a) says:

> For fifteen years—namely from 1843 to 1858, the *local* injection of Wood seems to have been more and more employed in Scotland. . . .

In 1872, Hunter writes:

> In 1843, Dr. Wood of Edinburgh, pointed out another local use for the syringes then employed for injecting caustics into naevi. . . .

Yet it is perfectly clear from Wood's original (1855) publication that it was ten years later, namely in 1853, that he introduced the subcutaneous use of the syringe. Moreover, he employed for this purpose a syringe (Ferguson's) that had been devised for a technique (intravascular injection of iron perchloride) which had not been introduced until the beginning of 1853. The clearness of the evidence on this point makes it all the more remarkable that Wood's invention should have been incorrectly antedated by 10 years, and that this error should have appeared in the literature within 10 years of Wood's first publication and persisted to the present day. The reason is to be found in Wood's second (1858) publication, in which the date of his first subcutaneous administration of morphine is given as "in the end of November, 1843." At first sight, the simple explanation of the discrepancy is that the earlier date was a misprint, and this is probably the true one. Further consideration makes it difficult to exclude absolutely a less charitable explanation.

Even if the inclusion of the earlier date in Wood's paper were an accident, there are circumstances which render his failure to correct it conspicuous, and give rise to the suspicion that he passively conspired to perpetuate the error. In the first place, the date 1843 would give Wood a clear priority over Rynd, whose attempt to introduce morphine solution directly to peripheral nerves was made in 1844. In the second place, Wood was quick to defend in detail his claims to priority, as has been shown by the account already given of his undignified exchange with Charles Hunter in the correspondence solumns of the *Medical Times & Gazette.* In the course of this correspondence, Hunter (1865c) writes:

> For fifteen years was the syringe used by Dr. Wood on the local principle before I ever used the instrument.

Hunter first injected morphine subcutaneously in 1858, and this therefore implies that Wood had been using his technique since 1843. In the same letter, written in 1865, Hunter refers to "a method originated twenty-three years ago by Dr. Wood"—which again leaves 1843 as the date of Wood's invention. In his reply to this letter, Wood (1865b) makes no reference to this error, although in such a controversy it is hardly to be supposed that each contestant failed to scrutinize the words of his adversary with the minutest attention. To the present writer, it seems more probable that Wood deliberately refrained from correcting an error that tended to favor his cause, than that he did not notice it.

*c. Miscellaneous Errors.* Of the numerous other minor inaccuracies in references to the history of hypodermic medication, the following may be mentioned:

Keys (1945), following Bartholow (1891), describes Lafargue's 1836 technique as follows: "Using a needle trocar, he injected morphine paste subcutaneously. . . ." He states that in "1853 Wood devised the modern type of metallic hollow needle. . . ."

Keys' whole account is most misleading, and makes it appear that Wood's contribution was little more than a minor technical improvement on a method introduced by Taylor and Washington.

Macht (1916) attributes to Rynd "the priority of discovery of hypodermic injection." He describes Rynd's crude implement as a "syringe." Macht also refers to Wood's "first report on the subject" as being 1853, and this has misled Terry & Pellens (1928) to suppose that Wood published a paper in that year.

## 12. The Benefits of Hypodermic Medication

Much has been said of the disagreeable consequences attendant upon the early employment of hypodermic medication, and it is perhaps fitting to say something also of its benefits. Carl Binz (1895), referring to the hypodermic injection of morphine, is an eloquent witness to the practical value of the technique in alleviating the horrors of the battlefield.

> It was on a memorable occasion that I first had extensive personal experience of the great advantages attendant upon this method of administering the remedy [morphine]. On the evening of the 16th of August, 1870, after the battle of Rezonville, near Metz, I undertook the charge of over 180 soldiers who had been wounded,

half of them severely, and admitted them into my field-hospital, the church of Gorze, which had been utilized for that purpose. The men were driven up to the flight of steps outside the church in ambulance waggons; here they had to be lifted, carried up the steps on stretchers, and laid on beds of straw in the church. In anticipation of the amount of suffering which would be attendant upon their removal from the waggons, and their carriage up the steps and to their beds, I had 200 c.c. of a 1 per cent. solution of morphine prepared. Before any wounded man was lifted out of the ambulance he had a syringeful (1 c.c.), or if needful a syringeful and a half of the solution subcutaneously injected. Ten minutes were allowed to elapse, and then each man was lifted up and carried to his bed in the church. The result of these injections was that no cry of distress was heard, and that the first night, which to wounded men is probably the most painful, was passed in complete repose.

Perhaps one of the greatest benefits resulting from hypodermic medication was that it encouraged physicians to think in terms of precise dosages of single active principles.

## 13. CONCLUSIONS

The following conclusions may be drawn from this study. They refer only to events which were steps in the continuous evolution of hypodermic medication, and do not take into account collateral developments, such as the therapeutic injection of solutions into abscess-cavities.

1. *Lafargue* systematically employed hypodermic medication, first (1836) by the "inoculation" of solid drugs as a paste, then (1861) by the subcutaneous implantation of pellets—a technique which was revived in 1937. Lafargue's intention was to exert a local action upon the peripheral nerves, although he recognized that absorption and systemic action occurred.

2. *Rynd* practiced (1844) the hypodermic administration of a solution of morphine, with the intention of exerting a local action upon the peripheral nerves. He published no observations on systemic effects. Rynd did not employ a syringe, and his procedure was a hypodermic infusion.

3. *Wood* devised the technique of hypodermic injection in 1853 and introduced (1855) it as a therapeutic procedure. His intention was to secure a local analgesic action on peripheral nerves, and although he clearly recognized the occurrence of systemic effects, he failed to appreciate their significance.

4. *Hunter* adopted (1858) Wood's technique of local injection. The occurrence of infection at the site of injection in his first two patients led him to make remote injections. He recognized (1858) that the remote

injections were as effective as the local injections, and correctly argued that the subcutaneous route was effective by reason of systemic absorption. Hunter introduced (1859) the word "hypodermic" to distinguish his use of subcutaneous injection as a mode of systemic administration from Wood's use of the same technique for its supposed local action.

5. It was at first assumed that the hypodermic injection of morphine would be free from the habit-forming property of orally-ingested opiates. Recognition of addiction to hypodermically-administered morphine was reluctant and belated, and there was for some years a widespread abuse of morphine injection.

6. The danger of sepsis as a result of hypodermic injection was generally discounted, and when sepsis occurred it was commonly attributed to multiple injections at the same site or to the use of irritating solvents.

7. Several retrospective claims to priority of hypodermic injection have been considered, but none had a direct bearing on the evolution of hypodermic medication.

8. The commonly-reiterated attribution of responsibility for the introduction of hypodermic injection to *Pravaz* is entirely without foundation. Some other persistent errors in the history of hypodermic medication have been refuted.

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## REFERENCES

ADAMS, E. W. (1937), *Drugs Addiction*, Oxford.

ALLBUTT, [T.] C. (1870), "On the Abuse of Hypodermic Injections of Morphia," *Practitioner* 5. 327-31.

ANSTIE, [F. E.] (1868), "The Hypodermic Injection of Remedies," *Practitioner* 1. 32-41.

ANSTIE (1871), "On the Effects of the Prolonged Use of Morphia by Subcutaneous Injection," *Practitioner* 6. 148-57.

ARNOLD & SONS (1875), *Brit. Med. Jour.* 2. 751.

ARNOLD & SONS (1876), *A Catalogue of Surgical Instruments*, London.

*Association Medical Journal* (1853), "Treatment of Aneurysm and Other Vascular Tumours by the Injection of a Solution of Perchloride of Iron," pp. 1082-85.

BARTHOLOW, R. (1891), *A Manual of Hypodermatic Medication; the Treatment of Diseases by the Hypodermatic or Subcutaneous Method*, 5th ed., Philadelphia.

BAY, G. (1879), *Gazette des hôpitaux* 52. 998.

BÉHIER, [L. J.] (1859), "Méthode Endermique: Injections Médicamenteuses Sous-cutanées." *Bull. gén. thérap. méd. chirurg.* 57. 49-61, 105-10.

BELL, B. (1858), "The Therapeutic Relations of Opium and Belladonna to Each Other," *Edin. Med. Jour.* 4. 1-7.

BERNARD, C. (1855), *Leçons de physiologie experimentale appliqué a la médecine,* Paris, pp. 213-14.

BERNARD, C. (1879), *Leçons de physiologie opératoire,* Paris, p. 192.

BERTRAND (1856). Cited by Erlenmeyer, 1887. According to Erlenmeyer, Bertrand published on hypodermic injection of morphine solution in *Correspondenzbl. d. deutsch. Gesellsch. f. Psychiat. u. gerichtl. Psychologie* 3 (1856) 119.

BINZ, C. (1895), *Lectures on Pharmacology for Practitioners and Students,* London. (Translated from the second German edition by A. C. Latham, and revised and corrected by Binz.)

BISHOP, P. M. F. (1938), "A Clinical Experiment in Oestrin Therapy," *Brit. Med. Jour.* 1. 939-41.

[BOURGUIGNON] (1860), "Nouvelle seringue à injections hypodermiques," *Bull. gén. thérap. méd. chirurg.* 59. 93-94.

BRAINARD, D. (1853), "Erectile Tumour of the Orbit, Cured by Infiltration with the Solution of the Lactate of Iron and Puncture with Hot Needles, after the Ligature of the Carotid Artery had Failed," *Lancet,* 2. 162-64.

BRERA, [V. L.] (1880). (Cited by Eulenburg, 1865, 1880.)

*British Medical Journal* (1875), 2. 675.

BROWN, T. (1886), *Alexander Wood. A Sketch of his Life and Work,* Edinburgh.

BRUNS, V. VON (1869), *Arznei-Operationen oder Darstellung sämmtlicher Methoden der manuellen Application von Arzneistoffen,* Tübingen.

CASTIGLIONI, A. (1941), *A History of Medicine,* New York. Translated and edited by E. B. Krumbhaar.

CHAUVET, S. (1936), *La médecine chez les peuples primitifs,* Paris.

CHRESTIEN, A. J. (1804), *De la méthode iatraleptique* . . . Montpellier. (Cited by Eulenburg 1865, 1880. A short note on this work also appears in E. J. Waring, *Bibliotheca therapeutica,* London 1878.)

CHRISTISON, R. (1886), *The Life of Sir Robert Christison, Bart.,* Vol. 2, Edinburgh. (Edited by his sons.)

COMRIE, J. D. (1931), *A History of Scottish Medicine,* 2nd ed., vol. 2, London.

CROMBIE, J. M. (1873), "A Simple Method for the Subcutaneous Application of Morphia," *Brit. Med. Jour.* 2. 194.

CUMSTON, C. G. (1926), *An Introduction to the History of Medicine,* London.

CURRIE, A. S. (1887), *Manual of Hypodermic Medication,* London. (Translated, with additions, from the second edition of Bourneville, [D. M.] & Bricon, [P.] *Manuel des injections sous-cutanées.*)

DEANSLEY, R. & PARKES, A. S. (1937), "Factors Influencing the Effectiveness of Administered Hormones," *Proc. Roy. Soc. B.,* 124. 179-97.

DURAND (1924), "Charles-Gabriel Pravaz," *Paris médicale* (Partie paramédicale) 54. 334-37.

ERLENMEYER, A. (1887), *Die Morphiumsucht und ihre Behandlung,* 3. Aufl., Berlin.

EULENBURG, A. (1865), *Die hypodermatische Injektion der Arzneimittel,* Berlin.

EULENBURG, A. (1880), *Percutane intracutane und subcutane Arzneiapplicationen* in Ziemssen, H. von, *Handbuch der allgemeinen Therapie.* I. 3, Leipzig.

FOOTE, G. A. (1877), *Trans. Med. Soc. North Carolina.* (Cited by Kane, 1880.)

FÜLÖP-MILLER, R. (1938), *Triumph over Pain,* London.

GARRISON, F. H. (1929), *An Introduction to the History of Medicine,* 4th ed., Philadelphia.

GAUJOT, G. (1867), *Arsenal de la chirurgie contemporaine,* Paris.

HUNTER, C. (1858a), "On Narcotic Injections in Neuralgia," *Med. Times Gaz.* 2. 408-409.

HUNTER, C. (1858b), *Med. Times Gaz.* 2. 457-58.

HUNTER, C. (1859), "A Series of Cases of Narcotic Injection into the Cellular Tissue in Neuralgia and Other Diseases," *Brit. Med. Jour.* 19-20. [Only 1 vol. in year.]

HUNTER, C. (1863), "Practical Remarks on the Hypodermical Treatment of Disease,"*Lancet* 2. 444-45, 675-76.

HUNTER, C. (1865a), *On the Speedy Relief of Pain and Other Nervous Affections by Means of the Hypodermic Method,* London.

HUNTER, C. (1865b), "Hypodermic Administration of Certain Medicines," *Med. Times Gaz.* 1. 584-87.

HUNTER, C. (1865c), *Med. Times Gaz.* 2. 22.

HUNTER, C. (1865d), *Med. Times Gaz.* 2. 107.

HUNTER, C. (1872), in *Cooper's Dictionary of Practical Surgery and Encyclopaedia of Surgical Science,* vol. 2, London. (Edited by S. A. Lane.)

JOUSSET, G. L. (1865), *De la méthode hypodermique et de la pratique des injections sous-cutanées,* Paris.

KANE, H. H. (1880), *The Hypodermic Injection of Morphia. Its History, Advantages and Dangers,* New York.

KEYS, T. E. (1945), *The History of Surgical Anesthesia,* New York.

LAFARGUE, [G.-V.] (1836a), "Notes sur les effets de quelques médicaments introduits sous l'épidermie," *C. R. Acad. Sci. Paris* 2. 397-98, 434.

LAFARGUE, [G.-V.] (1836b), "Lettre de M. le Docteur Lafargue de Saint-Emilion, sur l'inoculation de la morphine avec la lancette," *Bull. Acad. Méd. Paris* 1. 13-18.

LAFARGUE, [G.-1.] (1836c), "Lettre de M. le Docteur Lafargue de Saint-Emilion," *Bull. Acad. Méd.* Paris 1. 40-42.

LAFARGUE, [G.-V.] (1847), *Bull. gén. thérap. méd. chirurg.* 33. 182. (Passages from this paper are cited by Lafargue, 1861. The original has not been available, because of conditions resulting from the war.)

LAFARGUE, [G.-V.] (1861), "Inoculation hypodermique par enchevillement des substances actives telles que le sulfate de morphine, le sulfate de strychnine, dans le traitement des névralgies, des paralysies partielles . . ." *Bull. gén. thérap. méd. chirurg.* 60. 22-26, 150-59.

LAFITTE, L. (1875), "Des injections sous-cutanées d'eau distillées ou d'eau pure;—leur bons effets thérapeutiques," *L'Union médicale* 20. 445-48, 458-61.

LEITER, J. (1864), "Vereinfachte subcutane Injektionsspritze," *Wien. med. Wschr.* 14. 366-67.

[LEITER, J.] (1875), *Brit. Med. Jour.* 2. 674-75.

LELUT, E. (1875), "Les injections sous-cutanées d'eau distillée ou d'eau pure," *L'Union médicale* 20. 513.

LEMBERT, A. (1828), *Essai sur la méthode endermique, ou moyen thérapeutique particulièrement appliqué aux maladies nerveuses et rhumatismales,* Paris.(Cited

by Eulenburg, 1865, 1880. A short note on this work also appears in E. J. Waring, 1878, *Bibliotheca therapeutica,* London.)

LESIEUR, A. J. (1825), *Méthode endermique,* Paris. (Cited by Eulenburg, 1865, 1880. Eulenburg does not identify this publication further than to give the name of Lesieur and the date 1823. The initials, year, and title given above are taken from E. J. Waring, 1878, *Bibliotheca therapeutica,* London.)

*London Medical Record* 12 (1884) 276.

LUNDY, J. S. (1946), "Factors that Influenced the Development of Anesthesiology," *Current Res. Anesth.* 25. 38-43.

MACHT, D. I. (1916), "The History of Intravenous and Subcutaneous Administration of Drugs," *Jour. Amer. Med. Asso.* 66. 856-60.

MAGENDIE, F. (1809). (An excellent description of Magendie's experiments is to be found in J. M. D. Olmsted, *François Magendie: Pioneer in Experimental Physiology and Scientific Medicine in Nineteenth Century France.* New York, 1944.)

MALGAIGNE, J. F. (1874), *Manuel de médecine opératoire,* 8th edition by L. le Fort, Paris.

MARTIN-SOLON, [F.] (1836), "Lettre sur l'inoculation de la morphine et d'autres substances; par M. Lafargue de Saint-Emilion," *Bull. Acad. Méd. Paris* 1. 249-54.

MASSON, O. (1853), "Anévrisme poplité traité par l'injection de perchlorure de fer," *Gazette des hôpitaux,* 496-97.

MEDICAL AND CHIRURGICAL SOCIETY (1867), "Report of the Scientific Committee Appointed to Investigate the Physiological and Therapeutical Effects of the Hypodermic Method of Injection," *Med.-Chirurg. Trans.* 32. 561-643.

MEDICAL RESEARCH COUNCIL (1945), *The Sterilization, Use and Care of Syringes,* War Memorandum, No. 15, London.

*Medical Times & Gazette* 2 (1865) 337.

OLIVER, G. (1871), "On Hypodermic Injection of Morphia," *Practitioner* 6. 75-80.

PARIS, J. A. (1843), *Pharmacologia,* 9th edition, London.

PEREIRA, J. (1857), *The Elements of Materia Medica and Therapeutics,* vol. 2, part 2, London.

PRAVAZ, C.-G. (1853a), "Sur un nouveau moyen d'opéer la coagulation du sang dans les artères, applicable à la guérison des anévrismes." (Extrait d'une lettre de M. Lallemand à M. Rayer), *C. R. Acad. Sci. Paris,* 36. 88-91.

PRAVAZ, C.-G. (1853b), *Gazette des hôpitaux,* 237-238.

*Presse médicale* (1927), "Un monument à la memoire de Charles-Gabriel Pravaz," 35. 797.

RYND, [F.] (1845), *Dublin Med. Press* 13. 167-68.

RYND, F. (1861), "Description of an Instrument for the Subcutaneous Introduction of Fluids in Affections of the Nerves," *Dublin Quart. Jour. Med. Sci.* 32. 13.

SCHWIDETZKY, O. (1944), "History of Needles and Syringes," *Current Res. Anesth.* 23. 34-38.

SIEVEKING, E. H. (1861), "Clinical Remarks on Neuralgia," *Lancet* 105. [Only 1 volume in year.]

SUDHOFF, K. (1922), *Kurzes Handbuch der Geschichte der Medizin,* Berlin.

TERRY, C. E. & PELLENS, M. (1928), *The Opium Problem,* New York.

VEST, S. A. & HOWARD, J. E. (1939), "Clinical Experiments with Androgens. IV. A Method of Implantation of Crystalline Testosterone," *Jour. Amer. Med. Asso.* 113. 1869-72.

VILLARET, M. & MOUTIER, F. (1933), *La singulière histoire de l'injection intraveineuse*, Paris.

WEISS & SON (1843), *A Catalogue of Surgical Instruments and Apparatus*, London.

WEISS, J. & Co. (1863), *A Catalogue of Surgical Instruments, Apparatus, and Appliances* . . . London.

WOOD, A. (1855), "A New Method of Treating Neuralgia by the Direct Application of Opiates to the Painful Points," *Edinb. Med. Surg. Jour.* 82. 265-81.

WOOD A. (1858a), "Treatment of Neuralgic Pains by Narcotic Injections," *Brit. Med. Jour.* 721-23. [Only 1 volume in year.]

WOOD, A. (1858b), *Brit. Med. Jour.* 755.

WOOD, A. (1865a), *Med. Times Gaz.* 1. 639-40.

WOOD, A. (1865b), *Med. Times Gaz.* 2. 49-50.

YEO, T. BURNEY (1875), *Brit. Med. Jour.* 2. 702.

# John Ray and his Contributions to Plant and Animal Classification

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OF ALL the habits to which scientific men are susceptible, that of classifying the objects which come under their notice is probably the most persistent and certainly one of the most interesting. It is, therefore, not a little surprising to find that for almost two thousand years, what little study there was in the biological sciences made practically no advance in the field of systematic nomenclature; after the Middle Ages botany was hardly removed from the condition in which those giants of Grecian science, Aristotle and Theophrastus, had left it. The chief credit for initiating the progress which was then made, must go to an English divine, John Ray, whose long and honorable career has secured his place in the front rank of all scientists. A brief account of the work and progress of his immediate predecessors may indicate the legacy which he received and which he was so largely to enrich.

So long as the areas known to man's exploration were little broader than those known to the ancients, the amorphous classificatory systems of the latter were wholly adequate to the needs of the few students who touched on these subjects. But in the fifteenth and sixteenth centuries the humanistic revival of interest in classical writings was almost immediately followed by a vast increase in scientific research brought about by the geographical discoveries, and by an equally vast increase in the number of students which resulted from the rediscovery of the printing press. Thus a growing body of workers found a growing body of material impossible to manipulate within the limits of the Aristotelian system. Actually, it was long after zoology that botany emerged as an independent science, and in the earlier years of its revival it was, like chemistry, associated with the larger studies of medicine and pharmacology. The earliest botanical works were, therefore, either primitive pharmacopoeias or slight descriptions of plants known to the author. Of such a nature was the *Herbarum vivae eicones* of Otto Brunfels (1488?-1534). Not far removed but yet representing a certain advance was the *Historia stirpium* of Leonhard Fuchs (1501-1566). These works, though beautifully illustrated, can hardly be regarded as contributions to a science

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and the credit for the first attempt to deal with the subject systematically belongs to Andrea Cesalpino (1519-1603). He used the fruit and flower as distinguishing characteristics, and although his work is developed along rigid Aristotelian lines and blemished by futile anatomical comparisons between plants and animals, Cesalpino was the earliest botanist to base his work on a comparative study of forms. A later contemporary of his, Caspar Bauhin (1550-1624), was among the first to outline the counterpart of the modern "genus." A little later Joachim Jung (1587-1657) devised a splendid clarification of Cesalpino's system, but he suffered severely from the prevalent obscurantism of the Catholic Church and his work received an unmerited eclipse in so far as it was not published until after his death when newer ideas were already being developed. These were the chief workers in botany prior to Ray and his contemporaries. It is naturally impossible to ascribe to any few men the foundations of work which were laid over many decades by many workers. Nor would the botanists themselves, for the most part, have wished it. Each borrowed and adapted or improved the work of his predecessors and contemporaries. On the whole, the science awaited the arrival of newer and greater men, and one must agree with Linnaeus, who said, "the framing of a true system had previously been impossible, because the differences and agreements of forms were not sufficiently known."

John Ray was born in 1627 at Black Notley in Essex. His father was a blacksmith whose humble occupation would hardly have permitted his son to attend the University without financial aid. However the promise of future achievement was already prominent in Ray as a youth and came to the attention of Samuel Collins, the neighboring Vicar of Braintree. Through the good offices of the latter, Ray was enabled to obtain the assistance of a sizarship at Cambridge. At Trinity he showed considerable distinction in classics, theology, and mathematics and acquired that general education in literary and scientific subjects which has since given place to the restricted specializations of modern learning. He was elected a Fellow of his college and at the time of the Restoration was ordained. In preaching he avoided the inflated style which disfigured pulpit oratory at that period and we learn that his sermons were distinguished for the absence of "that enthusiastic stuff, which the sermons of the time were generally filled with."[1] But in 1662 his academic career was abruptly closed by his refusal to subscribe to the "Bartholo-

[1] Sir J. E. Smith.

mew Act" (Act of Uniformity) which denied the validity of the oath of the "solemn league and covenant." Since Ray could not appreciate the ability of one man's oath to nullify that of another, he was forced to resign his Fellowship, affording another demonstration of the paralyzing effect of religious bigotry on scientific progress. The rest of his life he passed as a private scholar, contributing to the proceedings of the newly founded Royal Society of which he was elected a member, traveling considerably, researching industriously, and compiling the voluminous output on which his fame now rests. His private life seems to have been exceptionally happy and his appealing personality attracted a circle of admiring and assisting friends. His beautiful and devoted friendship with Francis Willughby has no parallel in biological history unless it be that of Linnaeus and Artedi. Both were equally short-lived because Willughby, like Artedi, died before he had expressed his full genius. Ray married late, but married well, and his wife was "recommended by her character as well as by her person to the regard of her husband." His career in biology extended over fifty-five years during which time books never ceased to come from his pen. He displayed an equal felicity in Latin, the language in which he composed his scientific treatises, and in English, in which he wrote his numerous theological and miscellaneous productions. The infirmities of old age never dampened his enthusiasm, but rather he bitterly complained of the lessened activity which his afflictions made necessary. To his death he polished and revised his works and bemoaned the fact that at the time he most wished to make researches for a last edition of his *Methodus,* his legs did not permit him even to walk in the fields, much less to visit the London Gardens. He died in the midst of such activity at the age of seventy-seven in 1705 at Black Notley, whither he had removed in 1679 "for the short pittance of time he had yet to live in this world," as he then said.

Ray's interest in natural history was manifested at an early age. While still at Cambrige in 1660, he published the first systematic flora in England.[2] This was the *Catalogus plantarum circa Cantabrigiam nascentium,* the fruit of the many excursions he had enjoyed in the society of his friends and pupils. Shortly after leaving Cambridge he conceived the ambitious project of cataloguing the entire organic world in a systematic scheme. With this intention Willughby and he (financed by the former) set out on a tour of Europe; Willughby's particular prov-

[2] In justice to its author the publication in 1650 of William How's *Phytologia Britannia* must be noted, though the work is not comparable with that of Ray.

ince was the animals while Ray concerned himself more especially with the plants. The untimely death of his friend at the age of thirty-seven brought Ray the mixed blessings of a pension of £60 a year, the care of the education of his benefactor's children, and the duty of publishing Willughby's unfinished manuscripts. This last office Ray filled with a skill exceeded only by his modesty, which for many years disguised the tremendous amount of his work and original writing in the publication of the zoological side of their scheme. Part of this work was the *Ornithologia* which was, according to Ellison Hawks, "the first serious attempt at the classification of birds since the days of Aristotle."[3]

Ray's first actual contribution to the problems of classification was in his "Tables Concerning Plants" which he wrote for inclusion in *An Essay Towards a Real Character and a Philosophical Language* (1669) drawn up by the fantastic Bishop Wilkins. In the light of his later work it is hardly relevant and is interesting chiefly as the first point of association which Ray had with his great contemporary, Robert Morison (1620-1683), Professor of Botany at Oxford. Morison was a man of the highest attainments but he lacked completely those niceties of personal charm which endeared men to Ray. In fact, his splendid work in botany was severely injured by his repugnant and hostile criticisms of his contemporaries. He wrote long diatribes against what he called the "Hallucinationes" of Bauhin, whose *Pinax theatri botanici* was particularly reverenced at the time, and he proclaimed himself the messiah of botanical science; while refusing to acknowledge his patently obvious debts to Cesalpino, he accused Ray of plagiarizing his own scheme. This charge, however, is hardly allowable; for the obscurities of Morison's scheme were scarcely relieved by the little of it that was actually published in his lifetime—and the full revelation of his efforts, able as they were, did not come until twenty years after Ray's death. Few historians have revised the judgment of Tournefort who said of Morison, "On ne saurait assez louer cet auteur; mais il semble qu'il se loue lui-même un peu trop. . . . Il ose comparer ses découvertes à celles de Cristoffe Colomb," and of Ray, "Mons. Ray sans faire tant de bruit a beaucoup mieux réussi que Morison. Sa modestie est louable et l'Histoire des Plantes qu'il nous a donnée est une Bibliothèque Botanique."[4] A lengthy comparison of Ray and Morison is not relevant; the latter was Ray's greatest English contemporary and their mutual debt was great. Apart from the intrinsic

[3] E. Hawks, *Pioneers in Plant Study* (New York, Macmillan, 1928).

[4] Joseph Pitton de Tournefort (1656-1708), *Eiéments de botanique* (1694).

merit of his work, Morison forms an amusing diversion in a serious century and a striking contrast to Ray himself. The particular point of plagiarism noted, concerned the adoption by Ray of foliage forms as distinguishing characteristics but as Ray later modified his system, the question is not important.

The new scheme was first published in the *Methodus plantarum nova* in 1682, and though he changed the details occasionally and did not actually use the words "cotyledones" and "dictotyledones" until the edition of 1703, the essentials remained unaltered. The work is not a large one but well arranged and clearly developed. A short preface acknowledges his debts to others and gains the reader's confidence. Then, some pages devoted to describing the parts and their functions are followed by the main scheme itself. If the original *Methodus* of 1682 was a bare schematic outline, the same can hardly be said of the *Historia plantarum generalis* (1686-1704). This work, referred to by Linnaeus and Haller as "opus immensi laboris," comprised the entire botanical knowledge of the time on 2860 folio pages and listed about 18,600 distinct species. Ray's discussions of the various aspects of physiology, anatomy, reproduction, and taxonomy were completely exhaustive of the knowledge of his time. The work is the summation of all that preceded him and the foundation of much that succeeded him; it remained the most thorough work of its kind, indeed, the only one of its kind, until those of Linnaeus. His next large technical contribution was the *Synopsis methodica stirpium britannicarum* (1690). It is also perhaps the one which endured the longest in practical use, for it was the inseparable companion of all British naturalists for well over a century.[5] Although there had been descriptive books on plants before the time of Ray, in this work and in its predecessor restricted to the Cambridge district, Ray was the first to anticipate the modern "flora" as a systematic catalogue of the plants in a given locality. His last large undertaking in these fields was the *Stirpium Europeanarum extra nascentium sylloge* (1694), which is perhaps the first approach to the complicated subject of the geographical distribution of plants.[6]

Ray showed himself interested not only in taxonomy but in the fundamental principles, or what might be called the philosophy, underlying the science. The hope of all taxonomists has been a classification devel-

[5] It was finally superseded in the early nineteenth century by Sir J. Smith's *Flora*.

[6] The work itself is chiefly an anthology derived from the reports of European botanists.

oped along "natural" lines—the search for Morison's "nota generica" by which all plants may be equally, clearly, and naturally distinguished. The tendency of an equal number has been to compromise with a classification which, however artificial, is immediately practical. Ray realized that "nature refuses to be forced into the fetters of a precise system."[7] He therefore built his system on the ideal of a natural classification. He also realized, as he said, that "natura non facit saltus"; yet his system failed to endure because in applying this doctrine he did not see that it is as applicable to a vertical as to a horizontal arrangement. It is important to note, however, that in the field of taxonomy, though Linnaeus derived much from Ray, his was an essentially artificial, and in that respect, a retrogressive classification. The real successors of Ray in taxonomy were Adanson, Antoine Laurent de Jussieu, and to a lesser extent, Tournefort.

Ray carefully rejected classification by geographical distribution or by the uses to which plants are put by man. The latter had been a favorite device of the herbalists who listed their plants according to their "Virtues," a method which survived the disputes they had among themselves concerning these properties and which was not finally dispelled until some time later.

Having eliminated these artificial classifications, he proceeded to develop his own system. The divisions of this he based chiefly on the differences of the fruit, but also on those of the flowers; in this latter method he followed the lead of Cesalpino and Jung who had used the flowers in their classifications. To a much lesser extent Ray used the differences in the leaves. He realized that the use of any single character or set of characters in classification only broke up natural groups and he, therefore, used whatever characters he thought most useful in distinguishing the various different groups. He first divided the members of the plant kingdom into those bearing winter buds and those (as he thought) not bearing them. The first included "Arbores" and "Fructes" (Trees and Shrubs) and the second division, "Suffructes" and "Herbae" (Undershrubs and Herbaceous Plants). This division was an old Aristotelian convention from which Ray never freed himself although he was well aware of its limitations. For it he earned the derision of Rivinus[8] whose bitter contempt was badly hidden beneath the complai-

[7] Actual words of Linnaeus, but the idea of Ray.

[8] Augustus Quirinus Bachman (Rivinus) (1652-1723), the first after Jung to abandon division into "Trees" and "Herbs," but who otherwise developed a very artificial system.

sant civility of his Latin. The two, in fact, carried on a correspondence on this and other subjects and Ray adopted some modifications suggested by Rivinus though he never abandoned his chief divisions. As a matter of fact, Rivinus was worsted in the contest because the system he proposed to substitute, based on the corolla, was so manifestly artificial that it lent itself easily to the powerful attacks of Ray. Such disputations, like those with Morison were distasteful to Ray and alien to his retiring disposition. The next division of the "Herbae" was into "Imperfectae" and "Perfectae." The latter were in turn divided into "Monocotyledones" and "Dicotyledones," as were also the "Arbores." The system was thus arranged as follows:

A. Plantae Gemmis Carentes (Herbae et Suffructae)
  a) Imperfectae (4 classes)
  b) Perfectae
    i. Dicotyledones (binis cotyledonibus) (19 classes)
    ii. Monocotyledones (singulis aut nullis cotyledonibus)
B. Plantae Gemmiferae (Arbores et Fructae)
  a) Monocotyledones (1 class)
  b) Dicotyledones. (6 classes)

These divisions were then separated into classes and the classes into genera, of which he finally denoted one hundred and twenty-five. Though in the light of modern knowledge Ray's arrangements may seem crude, many of his "sectiones" (classes) are preserved as Natural Orders today. Of such are the Labiatae, Leguminosae, Graminae, Compositae, Cruciferae, and Umbelliferae. He recognized the group Stellatae (Rubiacea) as distinct from Umbelliferae though related to them.[9] His first class of Imperfectae, the Plantae submarinae, included sponges and Fucus. His third class in the same division contained Mosses, Lichens, Liverworts, Lycopods and Confervae under Musci. He divided the Compositae into four classes which would hardly be justified as genera, and his Pomiferae are excessively comprehensive. In the first sub-kingdom his last class was Anomalae incertae sedis, and in the second sub-kingdom the last class was Arbores Anomalae. Into these delightfully convenient receptacles he threw everything with which he was unable to cope otherwise.

Ray probably deserves less attention for the material he presented than for the manner of its presentation. His eye for detail and the ap-

[9] One of his debts to Cesalpino.

parently trivial was amazing. At the beginning of the *Methodus* we find him remarking on the fact that small seeds do not necessarily produce smaller plants than large seeds, and throughout all his writings there is a similar intention to examine and observe everything which came to his notice. His method was first to name and briefly diagnose his genus and then to give the names of the species in the genus followed by a brief description of each species. He was thus the first to describe both genus and species in the same place, in itself an important advance. Further, he was the first to recognize and define the general modern use of the word "species." In so far as when two similar animals mate and produce as their offspring a third similar animal, the three are regarded as belonging to one species, the denotation of the term has not been improved since the time of Ray, because this was his application of it. Similarly, the conception of the genus, if regarded as the limits within which animals may successfully fertilize one another (though in this case not necessarily producing similar offspring), has undergone little modification from the use Ray made of the term.[10] His distinction between dicotyledons and monocotyledons is fundamental; his failure to discern buds in anything but trees is distressing; there are many such contrasts of perception but the general merit of his work remains unaffected by them.

The considerable attention devoted to Ray's work in classification might encourage an underestimation of his talents in other fields. He lived in an age when the principles of anatomy and physiology were the subjects of widespread research. The work of such pioneers as Vesalius, Servetus, and Harvey was still exciting in his day and he was a contemporary of Malpighi and Leeuwenhoek. Furthermore, in the seventeenth century the microscope had achieved the greatest development it was to reach before the discovery of achromatic lenses and sub-stage condensers in the early nineteenth century. Ray was quite familiar with such works as Hooke's *Micrographia.* He was inspired by all he saw and heard. His own researches on sap in trees are classical and many of them anticipated the later elaborate researches of Stephen Hales. Ray and Willughby were the first to demonstrate (among other points in this matter) that "the sap ascends, not only between the bark and tree but by all the pores of wood." He even searched for valves in the wood. He

[10] Since sex in plants was just being acknowledged at the time, the illustrations are from the animal kingdom, but Ray's conceptions of genus and species in plants was little different. However, the idea of genus in plants had been previously developed by Bauhin.

may not have understood the intimate details of photosynthesis but he realized the significance of both light and air in nourishing the tree and understood the reason for the greater growth of a girdled tree above the wound than below it. He was a pioneer also in his work on the functions of winter buds and their parts, and no less so in his descriptions of germination and of the endosperm, called by him the "medulla." The discovery of sex in plants has been variously ascribed to Nehemiah Grew[11] and Camerarius;[12] but four years before Camerarius published, Ray had expanded the incoherent conceptions of the former and in his *Synopsis* he wrote, "hence indeed is confirmed the opinion of those who teach that the dust contained in the apices of the stamens performs the function of the male." It is interesting also to find him taking up a determined stand against the "atheist's" theory of spontaneous generation, and he discussed the whole question with great acuteness in several of his works.

Much might also be written of his views on mutation. In a letter to Willughby he described two reports of apparent transmutation, adding "adeo ut jam persuasus sim plantas degenerare posse et intra latitudinem generis seu tribus suae speciem mutare." In the *Historia* the question is discussed at greater length. However, he would not allow that new species were developed but only that one species might "degenerate" into another already existing species. He was familiar with what little was known of hybridization, and he did not regard the different colored flowers in plants as separate species since the color variations are not reproduced through seed but only through cuttings. Although he has been cited as the prophet of evolutionary theories, it is a mistake to attribute to him any profound knowledge of phylogenetic principles. Actually, his opinions, though by no means narrow, were teleological. But it was necessary for someone to teach, as Ray did, that "Nature makes not a long neck to no purpose," before Darwin and Wallace could show how She makes it.

Since one of Ray's greatest ambitions, and in part his achievement, was the unification of all living things under one system, it is difficult to separate his zoological work from his botanical studies. Zoologists, however, may claim with much justification that the more valuable and lasting portion of his work lay in their science. It is difficult to segregate even his theological writings, for his intense love of natural history per-

[11] Nehemiah Grew (1628-1712), one of the two great vegetable anatomists of the seventeenth century, Malpighi being the other.

[12] Rudolph Jacob Camerarius (1665-1721), author of *De sexu plantarum* (1694).

vaded all these also, and his reverential attitude toward life in all its manifestations is itself awe-inspiring. His *Itineraries* are far more thrilling than the voyages of disillusionment and despair recorded by modern travelers. One of the most fascinating of his works is entitled *A Collection of English Proverbs,* "digested into a convenient method for the speedy finding one upon occasion; with short annotations. Whereunto are added local proverbs, with their explications, old proverbial rhythmes, less known, or exotic proverbial sentences and Scottish proverbs. Enlarged by the addition of many hundred English, and an appendix of Hebrew proverbs, with annotations and parallels." This work, the reading of which in every way justifies the interest evoked by the title, has been reprinted many times.

As a traveler Ray was much more than a botanical tourist absorbed only in the addition of a rare item to his catalogues. He was intensely interested in the dialects of the people he visited, and published the *Collection of English Words* in which he left behind a valuable account of local language habits. His observations of the customs of the people were extraordinarily astute and, very rarely, even a little acrid. He preceded that other great traveler, Dr. Johnson, in his condemnation of the Scots and said "their butter is very indifferent and one would wonder how they contrived to make it so bad."[13]

Of all the books published by Ray perhaps the most fascinating is *The Wisdom of God Manifested in the Works of the Creation* which was first published in 1691 and ran through several very successful editions even before Ray's death. In this work Ray departed from his descriptive and classificatory treatises and gave us a volume of interpretations; he passed from observation to induction and attempted to relate particular phenomena to general laws. The book was filled with a wealth of lore and science delivered in the most elegant English; the flow of his enthusiasm for natural studies was controlled only by the mixture of scientific logic and quiet humility which was his peculiar gift. The stature of the man is further increased in the latest edition (1848) by the extraordinarily trivial annotations which his editors felt obliged to add. He discussed the goose barnacle myth and the use of kelp as manure; he ranged from a consideration of the nature of atoms to notes on the influence of the moon upon tides. He pondered the hexagonal shape of bees' cells and remarked upon the factors governing the number of eggs laid by birds. He was the first to show the true origin of fossils as the

[13] Quotation from the *Itineraries.*

petrified remains of extinct species. The work, however, was much more than a collection of casual jottings. It was a very great attempt to co-ordinate the biological knowledge of the day into a system acceptable alike to the scientists and the theologians of the times. Ray himself belonged to both groups and he felt no embarrassment or inconsistency in this position. If he insisted that revealed religion could be illuminated by a study of nature he was equally convinced that such a study alone would not explain all the phenomena observed. Of the significance of this work in the development of scientific thought Raven says, "more than any other single book it initiated the true adventure of modern science, and is the ancestor of the *Origin of Species* or of *L'Évolution Créatrice*."

It would be pleasurable to quote at length from these writings whose delights cannot otherwise be conveyed. It would thus, indeed, be possible to accumulate the contributions and augment the importance of Ray in scientific and literary progress. Yet this would have been the last intention of the man himself. It is enough to say that by developing the use of anatomy as a descriptive feature for the classification of the organic world he introduced a more modern tone to scientific writing in general: if in his detail he often erred, in his delineation of the principles and philosophy which should guide botanical and, in fact, all scientific research he was completely successful.

The age in which he lived was characterized by almost as much turmoil and bitterness as our own, and this age Ray stood out conspicuously among scientists for the breadth of his vision and the generosity of his spirit, affording an example valuable to all who have succeeded him. "The character of Mr. Ray cannot be contemplated by those who have a true relish for the studies of nature without a high sentiment of respect and gratitude, nor by those who consider the exemplariness of his life as a man, and his qualifications as a divine, without veneration."[14] His philosophy and approach to science we may study in one of his youthful letters where he wrote, "I resolve never to put out anything which is not as perfect as it is possible for me to make it"; a further insight is given by a passage from *The Wisdom of God* in which we read, "I am so well acquainted with the Credulity of the Vulgar, and the Delight they, and many of the better Sort too, have in telling of Wonders,

[14] R. Pulteney, *Sketches of the Progress of Botany in England* (London 1790) 1.280.

and strange Things, that I must have a Thing well attested, before I can give a firm Assent to it." Few men before and few since have framed ideals as high as these. He considered it the privilege and the duty of all men to regard and study the works of nature. And so his place in science is undoubtedly inferior to his place among scientists for after all the great achievement of his life was (as he said himself in another connection) that "many were prompted to those studies and to mind the plants they met with in their walks in the fields."

## SOME REFERENCES TO THE LIFE AND WORKS OF RAY


1. ARBER, AGNES. *Herbals, Their Origin and Evolution.* London, Cambridge University Press, 1912. Authoritative for Ray's predecessors; contains only passing references to Ray himself.
2. DERHAM, GEORGE. *Life of John Ray.* Derham knew Ray during the latter part of his life. His biographical papers on Ray were edited in 1760 by Scott under the title *Select Remains of the Learned John Ray.* The Ray Society later published this life and other biographical accounts in one of their volumes.
3. HARVEY-GIBSON, R. J. *Outlines of the History of Botany.* London, A. & C. Black, Ltd., 1919.
4. HAWKS, ELLISON. *Pioneers in Plant Study.* New York, Macmillan Company, 1928.
5. MACGILLIVRAY, W. *Lives of Eminent Zoologists from Aristotle to Linnaeus.* Edinburgh, Oliver & Boyd, 1834.
6. NORDENSKIÖLD, ERIK. *History of Biology.* Translated from the Swedish by Leonard Bucknall Eyre. New York, Tudor Publishing Company, 1935.
7. OLIVER, F. W. (Editor). *Makers of British Botany.* Cambridge, Cambridge University Press, 1913. The chapter on Morison and Ray is by S. H. Vines who said, however, that Ray failed to apply his cotyledon division to trees.
8. PULTENEY, RICHARD. *Historical and Biographical Sketches of the Progress of Botany in England from its Origin to the Introduction of the Linnaean System.* 2 volumes, London, 1790. Volume 1 devotes several chapters to Ray, largely derived from Derham's *Life of John Ray.*
9. RAVEN, CHARLES E. *John Ray, Naturalist, His Life and Works.* London, Cambridge University Press, 1942. Dr. Raven has created the definitive biography of Ray and the immense labor which must have gone into his tribute to Ray will be at least partially repaid by the gratitude of later scholars. He has been particularly skilful in portraying Ray against the social, scientific, and theological background of his times. The work contains an exhaustive list of original references.
10. SACHS, JULIUS. *History of Botany* (1530-1860). Authorized translation by Henry E. F. Garnsey; revised by Isaac Bayley Balfour. Oxford University Press, 1906.
11. SINGER, CHARLES. *A Short History of Biology.* Oxford, the Clarendon Press, 1931.
12. SMITH, SIR. J. E. *Life of John Ray,* in Rees' *Cyclopaedia.* London, 1819. This account was also published along with reference No. 2 in the volume of the Ray Society mentioned above. It is largely derived from Pulteney and Derham.

# *Notes and Queries*

*Edited by* MAX H. FISCH*

## *Anna B. Kuster Welty, No. 5601*

THE STOCKTON STATE HOSPITAL is now nearing the century mark of its service to the mental patients of California. Created as a general hospital by the legislature of the state in May, 1851, and reconstituted in 1853 as the State Insane Asylum, its annual admissions increased in the first nineteen years of its existence from 13 to 482, with an over-all total in 1869 of 4596. There was a corresponding growth in the physical plant of the institution, and when the Stockton City Directory published in its First Annual Issue in 1870 the statistics of admissions and discharges, it also mentioned, with a note of pride, the beautiful location of the asylum, its hundred and thirty acres of excellent land devoted largely to neat and tasteful gardens, and the magnificent structure for female patients which had been erected some three or four hundred yards distant from the principal building.[1]

The Medical Superintendent of the asylum during the period 1865 to 1883 was Dr. G. A. Shurtleff; his Assistant Physician from 1870 to 1875 was Dr. I. S. Titus. On Feb. 8, 1873, on complaint of Dr. A. B. Nixon, Judge Robert C. Clark of Sacramento County committed to their care one Anna Welty, aged 50, single and without property, a Swiss servant who had lived in California for twenty years. Irrational conduct and conversation, induced a few weeks earlier by domestic trouble, had rapidly developed into mania; by the time of her commitment she showed no rational intervals and was considered dangerous to her own safety. Two days later she was admitted as the 5601st patient of the institution.

But in her own eyes Anna Welty was entirely rational. The Army Medical Library possesses a curious and pitiful document which issued from her hand some four years after her commitment. It is a piece of cotton cloth, 297 x 440 mm. in size,[2] now yellowed and stained, on which she had painstakingly sewed in brown thread her plea for release. To whom the letter was addressed is unknown;[3] how it reached the world outside the asylum is likewise a matter for conjecture. Composed in language which is a combination of incorrect German and phonetically spelled English, and sprinkled with pas-

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[1] For this information from the Stockton City Directory, for the material which follows from case histories in the files of the Stockton State Hospital, and for permission to publish this article I am deeply indebted to Dr. R. B. Toller, Superintendent and Medical Director of the Stockton State Hospital.

[2] It can hardly be a handkerchief, since it is not square and since linen was used for handkerchiefs at that time. It was possibly torn from a sheet.

[3] Dr. Toller suggests the President of the United States, who at the time the letter was composed was Rutherford B. Hayes.

sages which reveal the psychotic tendencies of its writer, it none the less tells its own story so graphically that it is here reproduced in photograph (Fig. 1), in transcription, and in translation with a minimum of commentary.[4]

STOKTON UNSINN. HAUS DEN 11. M/ 1877./ HOCHGEA. HERR./ ICH BITTE IHNEN MICH ZU ENTSCHULDIGEN DASS/ ICH DIE FREIHEIT NEHME AN IHNEN AUF EIN TUCH/ ZU SCHREIBEN. ICH BIN XXV. JAHR IN CAL. UND/ KANN BEWEISEN DASS ICH XXI. JAHR HART GEAR/BEITET HABE UND THU AUCH HIER ARBEITHEN./ ZU DIESER WAARD GEHÖREN VIII. ZIMMER EIN HAUS/GANG UND EINE TREPPE. ICH HABE DIE GANZE/ WAARD UND ALLE ZUGEHÖRDEN AUF MEINEN KNIE/EN GESCHEUERT GESGROBT AUSGENOMMEN DER BÖ/SEN EMMA ADCHENSEN IHR ZIMMER. DIESE EMMA/ WOLLTE DER GROSSE HERR DOCTOR HEIRATHEN/ UND MISSES RHEIT HAT IHM AUCH DER HOF GE/MACH UND WEIL ER KEINE GENOMMEN HAT EM/-MA UNTER IV. VERSCHIEDENEN MALEN DURCH/ DIE FENSTER GESCHREIT ZU H. ONER GARTNER/ H. DOCTOR TAITUS WUD NOT MARI WAN

Stockton Insane Asylum, Mar. [May?] 11, 1877. Highly esteemed sir, I beg of you to pardon me for taking the liberty of writing to you on a piece of cloth. I have been in California for 25 years and can prove that I have worked hard for 21 years and that I am also working here. In this ward are included 8 rooms, a hall, and a staircase. I have scrubbed the whole ward and everything belonging to it on my knees with the exception of the room of the bad Emma Adchensen [Atkinson].[5] This Emma wanted to marry the important doctor, and Mrs. Rheit [Wright?][6] was also playing up to him, and since he took neither one Emma 9 different times screamed through the window to Mr. Oner[6] the gardener, "Dr.

[4] Mr. David Gaehr of Hudson, Ohio, and Mr. Jean Eschmann of the Army Medical Library have given material assistance in the interpretation of some of the more obscure passages. Absolute clarity can hardly be expected from the diseased mind which composed the document.

[5] Emma Rose Atkinson was first committed to the asylum on Aug 24, 1865, at the age of 24. A native of eastern Canada, she had fallen victim to an attack of fever which affected her mind. Her insanity showed itself in wild and incoherent language, in singing, and in disposition toward poetry and love; she displayed no homicidal or suicidal tendencies. Discharged on November 1 of the same year, she was re-committed under the name of Emily R. Atkinson on Apr. 27, 1866, after an attempt to choke herself with a rope. She suffered from the delusions that her mother was upstairs dead, that her brother and sister had also died, and that someone wanted to kill her. Twenty years later she died in the asylum of consumption of the bowels.

[6] The Stockton State Hospital has no information in its files concerning this individual.

U.S.W./ TRU DA WINDO. EMMA UND M. ONIL HABEN NACH/HER DEN H. DOCTOREN GESAGT ICH HABE DAS/GETHAN DESWEGEN IST HERR DOCTER TAITUS/ BÖS. M. ONIL HAT IHM ZWEI TAG OB ER FORT/ IST GESAGT ER SOLLE MISSES RHEIT NEHMEN/ IHRE VERWANDTE. DIESE NÖRS HAT ZU H. DOC/-TER SCOTLOF GESAT ER SOLLE MICH NICT HER/AUS LASSN. ICH BIN HIER UBER VIER JAHR BIN/ NICHT UNSINNIG. ICH BITTE IHNEN HOCHGEA./ MIR AUS DIESEM HAUS ZU HELFEN. ICH WILL/ IHNEN BEZAHLEN AUS DER MUNY BANK. ICH HA/BE DREY BRUDER BEI PHILADELPHIA DIESE SEIN/ ALLE GUT AB. ICH VERLANGE NACH DEN STAATEN/ ZU GEHN. DAS GESPRACH WEGEN HERRN DOC/TER TAITUS KOMMT VON DEN EIRISCHEN ICH/ HBE NIE KEIN CENT AUF SCHLECHTE ART GEMACHT. MIT HÖCHSTER ACHTUNG/ ANNA B. KUSTER WELTY.

Taitus [Titus] would not marry one etc.," through the window. Emma and M. Onil [O'Neill?][6] told the doctor later that I did it and so Dr. Taitus [Titus] is angry.[7] M. Onil [O'Neill?] two days before he went away told him he ought to take Mrs. Rheit [Wright?], her relative. This nurse told Dr. Scotlof [Shurtleff] he should not let me out. I have been here over four years and am not insane. I beg of you, highly esteemed sir, to help me out of this asylum. I will pay you from the money bank. I have three brothers near Philadelphia. They are all well off. I demand to go to the states. The talk against Dr. Taitus [Titus] comes from the Irish. I have never made a cent wrongly. With highest respect, Anna B. Kuster Welty.

The years crept on, and Ann Welty's hope of intercession was not realized. The closing scene of the tragedy moves swiftly and starkly. On Oct. 29, 1884, she escaped from the asylum. The following day she was found drowned near San Joaquin Bridge, twelve miles distant. It was the opinion of the authorities that the drowning was probably accidental.

DOROTHY M. SCHULLIAN

Army Medical Library, Cleveland, Ohio

QUERY No. 10. Who was Joseph Dalby?

The only preface John Baskerville, the great eighteenth century English

[7] Dr. Titus had resigned from his position at the asylum two years before this letter was composed.

STOKTON UNSINN. HAUS DEN 11. M
1872.

HOCHGEA. HERR.

ICH BITTE IHNEN MICH ZU ENTSCHULDIGEN DASS ICH DIE FREIHEIT NEHME AN IHNEN AUF EIN TUCH ZU SCHREIBEN. ICH BIN XXV. JAHR IN CAL. UND KANN BEWEISEN, DASS ICH XXI. JAHR HART GEARBEITET HABE UND THU AUCH HIER ARBEITHEN. ZU DIESER WAARD GEHÖREN VIII. ZIMMER EIN HALS GANG UND EINE TREPPE. ICH HABE DIE GANZE WAARD UND ALLE ZUGEHÖRDEN AUF MEINEN KNIEEN GESCHEUERT GESGROBT AUSGENOMMEN DER RÖSEN EMMA ADCHENSEN IHR ZIMMER. DIESE EMMA WOLLTE DER GROSSE HERR DOCTER HEIRATHEN UND MISSES RHEIT HAT IHM AUCH DER HOF GEMACH UND WEIL ER KEINE GENOMMEN HAT EMMA UNTER IX. VERSCHIEDENEN MALEN DURCH DIE FENSTER GESCHREIT ZU H. ONER GARTNER H. DOCTER TAITUS WUD NOT MARI WAN U.S.W. TRU DA WINDO. EMMA UND M. ONIL HABEN NACHHER DEN H. DOCTOREN GESAGT ICH HABE DAS GETHAN DESWEGEN IST HERR DOCTER TAITUS BÖS. M. ONIL HAT IHM ZWEI TAG OB ER FORT IST GESAGT ER SOLLE MISSES RHEIT NEHMEN IHRE VERWANDTE. DIESE NÖRS HAT ZU H. DOCTER SCOTLOF GESAT ER SOLLE MICH NICT HER AUS LASSN. ICH BIN HIER ÜBER VIER JAHR BIN NICHT UNSINNIG. ICH BITTE IHNEN HOCHGEA, MIR AUS DIESEM HAUS ZU HELFEN. ICH WILL IHNEN BEZAHLEN AUS DER MUNT BANK. ICH HABE DREY BRÜDER BEI PHILADELPHIA DIESE SEIN ALLE GUT AB. ICH VERLANGE NACH DEN STAATEN ZU GEHN. DAS GESPRACH WEGEN HERRN DOCTER TAITUS KOMMT VON DEN EIRISCHEN ICH HBE NIE KEIN CENT AUF SCHLECHTE ART GEMACHT.

MIT HÖCHSTER ACHTUNG,
ANNA B. KUSTER NVELTY.

# THE VIRTUES
OF
# CINNABAR AND MUSK.
Againſt the BITE of a
# MAD DOG,
ILLUSTRATED,

134

In a Letter to Sir *GEORGE COBB*, Baronet.

In which are recited upwards of a hundred Caſes, wherein this Medicine hath happily ſucceeded: (whereof two were after the *Hydrophobia* appeared;) together with ſome few Inſtances wherein it hath not ſucceeded; owing entirely to its not being properly given, as is proved to Demonſtration: with proper Directions whereby to avoid the like Diſappointment for the Future.

WITH A WORD OR TWO

Concerning Dr. *HENRY BRACKEN*'s newly diſcovered *Specifick*, of near eighteen hundred Years ſtanding; being a ſufficient Refutation of the flimſy Arguments advanced by this celebrated Writer, in a learned Rant on the Virtues of *Gooſe-greaſe*.

By *JOSEPH DALBY*, Surgeon.

*Cauſa latet, vis eſt notiſſima.*

*BIRMINGHAM;*
Printed by JOHN BASKERVILLE, for the Author:
and Sold by Z. STUART, in Pater-Noſter-Row, London;
Meſſ. FLETCHER, PARKER, and PRINCE, in Oxford.
MDCCLXII.

printer, ever wrote was for his famous edition of Milton's *Paradise Lost* published in the year 1758. In the last paragraph of this preface, he says:

> It is not my desire to print many books; but such only, as are books of Consequence, of intrinsic merit, or established Reputation, and which the public may be pleased to see in an elegant dress, and to purchase at such a price, as will repay the extraordinary care and expense that must necessarily be bestowed upon them. . . .[8]

Most of the publications of the Baskerville press were editions of the Bible, the Book of Common Prayer, and the classics. Among the one hundred and eleven titles known to have been printed under Baskerville's personal supervision, there are two within the field of medicine. This statement is contrary to the one made by some medical historians that William Hunter's monumental work *The Anatomy of the Human Gravid Uterus,* Birmingham, 1774, is the only medical publication from the Baskerville press.[9]

The Army Medical Library has, in addition to the Hunter work, a small treatise entitled *The Virtues of Cinnabar and Musk against the Bite of a Mad Dog* by Joseph Dalby, Surgeon. Birmingham: Printed by John Baskerville, for the Author, MDCCLXII (see illustration).

> Collation: Title, advertisement on verso, subscribers 6 unnumbered pages, text pages 3-40, supplement pages 41-55, "To the Public . . ." on verso of page 55. Quarto.

As far as we can determine, this is the only work of Dalby's ever published. The book evidently had popular appeal at the time of its appearance as a second printing came from the same press in 1764, two years from the date of the first issue. It would be interesting to know why and how the discriminating Baskerville chose to publish this particular manuscript. Perhaps someone can help us identify the author or submit other data which might give added interest to Dalby's discourse.

ROBERT B. AUSTIN

Army Medical Library, Cleveland, Ohio

### *History of Science Essay Prize*

The History of Science Society takes pleasure in announcing the establishment of a History of Science Prize to be awarded annually to the undergraduate or first year graduate student in an American or Canadian college, university, or professional school who shall submit the best essay or original paper on some aspect of the history of modern science. The prize is $100. The donor (who prefers to remain anonymous) desires that the subject of the prize essay be selected from the period 1600-1915, though in some instances the subject may be carried into the contemporary period.

The donor and the members of the Prize Committee have agreed upon the following contest rules:

[8] Douglas C. McMurtrie, *The Book* (Oxford Univ. Press 1943) 376.

[9] F. H. Garrison. *History of Medicine* (4th ed., Philadelphia 1929) 340; F. H. Garrison and L. T. Morton, *A Medical Bibliography* (London 1943) 338; Arturo Castiglioni, *History of Medicine* (New York 1941) 596.

1. The essay may treat any of the natural sciences or mathematics, in their pure or applied aspects, or the fields of engineering or agriculture. Papers on medical subjects (i.e. the clinical aspects of medicine) will not be considered.
2. It should deal primarily with the development of the principles or methods of science, with the relationship of its various branches, or with the connections between science and the ideas and events of the period under consideration.
3. The contest is open to upperclassmen and first year graduate students who are majoring in scientific or technical fields, or who are otherwise specially qualified. It is desired that in most instances the essays be transmitted by some member of the faculty who has deemed them worthy of submission. The essay should be accompanied by a brief statement of the student's formal training in science.
4. The essay must be written in English; it should be submitted in typed or otherwise clearly legible form; it must not exceed 6000 words in length (twenty double-spaced typewritten pages) and should not in most cases exceed 3000 words.
5. The paper must be properly documented, and include essential footnotes and a bibliography of the sources consulted.
6. All communications should be directed to the Chairman of the Prize Committee. The essays themselves must ordinarily be submitted before August 1 of the year the prize is to be awarded. The winner of the contest shall be barred from competing in subsequent years.

It is hoped the winning essay will be announced each year at the annual (December) meeting of the History of Science Society. If, however, no worthy essay is submitted no award will be made. The Prize Committee will take steps toward securing the publication of the winning essay in an appropriate scientific journal.

The contest is opened by the publication of this announcement, and for the first year entries will be accepted until October 1, 1947.

The members of the Prize Committee are:

Chairman: Henry Guerlac, Professor of History of Science, Cornell University, Ithaca, New York

I. Bernard Cohen, Harvard University, Managing Editor of *Isis*

John W. Streeter, Department of Astronomy, Brown University

JOHN F. FULTON

President, History of Science Society

### *Mary Soper Pope Medal*

The first Mary Soper Pope Medal of the Cranbrook Institute of Science, Michigan, has been awarded to Dr. Frans Verdoorn, Editor of Chronica Botanica, of Waltham, Mass., in recognition of his editorial and international relations work in biology as well as for his researches in cryptogamic botany and the history of the plant sciences.

### *Centenary of the Sheffield Scientific School*

Dr. John F. Fulton, a member of the Editorial Board of the JOURNAL, is writing a biography of Benjamin Silliman, the great nineteenth century American scientist and founder of the Sheffield Scientific School at Yale University.

The book will be brought out this year, the centenary of the School's founding, in the "Life of Science Library," the new series in the history of science published by Henry Schuman, Inc.

There will be a large exhibit in the Yale University Library of early scientific instruments with displays of books illustrating the history of the various sciences.

* * * * *

Dr. Josiah C. Trent, a member of the Editorial Board of the Journal, has recently been elected a member of the Charaka Club.

# Book Reviews

MARSHALL A. BARBER. *A Malariologist in Many Lands.* With a foreword by Paul F. Russell. Lawrence, Kansas: University of Kansas Press, 1946. 158 pp. $2.50.

THE distinguished malariologist, Dr. Marshall A. Barber was born in Indiana in 1868, twelve years before Laveran discovered the plasmodium of malaria. In 1911, after a long preliminary period as student and instructor at Harvard and the University of Kansas, he embarked on a career in public health. He served with the famous Philippine Bureau of Science, the United States Public Health Service, and the International Health Division of the Rockefeller Foundation. During the past few years, despite the physical infirmities of age, he has acted as consultant in tropical medicine to the Secretary of War and as lecturer at the Army Medical School. Successive chapters of the book deal with first-hand observations in the United States, Central America and the West Indies, the Philippine Islands, Malaya, West Africa, the Balkans, Russia, and other regions. Inasmuch as *A Malariologist in Many Lands* is intended to be the record of Dr. Barber's work and not the story of his life, autobiographical detail has been kept to a minimum and the personality of the author does not come into the foreground. The book is pleasantly mild in tone and manages to be concise and clear even in the exposition of technical minutiae.

Almost at the beginning of his slender memoir Dr. Barber discusses the great decrease in the incidence and extent of malaria in the United States. Readers of this JOURNAL who remember Dr. Erwin Ackerknecht's recent treatise *Malaria in the Upper Mississippi Valley* will study with interest and benefit the difference between the malariologist and the historian in their treatment of the theme. Dr. Barber reminds us that an analogous decrease in malaria has occurred in northern and central Europe. Moreover the decrease is almost universal in the United States and has occurred in many localities where no deliberate preventive measures were taken. While screening had much to do with the decrease in the United States, a simultaneous decrease occurred in parts of Europe where screens were little used. As to the influence of agricultural drainage, detailed study of different parts of the United States reveals conflicts and complications in the evidence. For example in eastern Arkansas certain large swampy regions situated to the east of the Crowley Ridge were drained as a prelude to the cultivation of cotton. Infested lowlands were thereby dried and the anopheline population was reduced. In a

flat region situated to the west of the selfsame Crowley Ridge, the cultivation of rice was introduced and the anopheline population increased. In *both* regions malaria decreased greatly. Dr. Barber summarizes his opinion in the following words:

In comparing the factors which contributed to the decline of malaria in the United States, we find it difficult to say which is of most weight; for they vary greatly according to time and locality. In one region malaria diminished when the land was drained, in another when it was flooded and devoted to rice cultivation; in a third when the woodlands were cleared, and in a fourth when an almost treeless prairie was cultivated. Common to all these changes was an almost universal improvement in the way of life of the people which brought with it more screens, more quinine, and more doctors to prescribe quinine. Better food and housing which followed the development of the land certainly helped people to resist all manner of disease. In many localities, usually in more recent times, conscious measures such as anti-malaria drainage and insecticides contributed to the decline of the disease. But considering the great bulk of the decrease in malaria which has been noted all over the country from the Great Lakes to Texas and through many years, I am inclined to give the greater credit to screens, quinine, and improved medical treatment.

In 1934 Dr. Barber went to Russia. Traveling in the company of another famous malariologist, Dr. L. W. Hackett, he visited Moscow, Saratov, Astrakhan, the Caucasus, Georgia, and the regions near Odessa. He was received favorably by scientists and officials and was enabled to collect specimens and observe methods. His comments are significant:

I have seen no country where there was so much anti-malaria activity as in Russia in 1934; and I assume that activity continued undiminished through the subsequent years. I venture the prophecy that Russia will lead the world in anti-malaria work after this war; not only because she will control so much of the world where malaria prevails but also because of the activity, skill, and enthusiasm of her scientific workers.

These observations on Russia are one sample culled from Dr. Barber's decades of observant travel through the malarious areas of the world. But if the author's varied experiences are unusual in their combination of local detail and geographic breadth, their extent and position in the dimension of time are no less noteworthy.

Dr. Barber has lived through the entire lifespan of modern malariology. In the eighteen-sixties and seventies, the Middle West was emerging from its notoriety as a malarious graveyard. Toward the end of the nineteenth century Laveran, Ross, and MacCallum discovered the fundamental facts about the plasmodium and its life-cycle. In the opening decades of the twentieth century Sir Malcolm Watson and others elaborated the first principles of rational control. In 1915-17 Dr. Barber had the good fortune to watch Watson

at work in the plantations of Malaya. In 1920 Barber joined the United States Public Health Service and began his major efforts in malariology. Much of his research was carried out with the aid of the Rockefeller Foundation. His most conspicuous contributions—inconspicuously and unobtrusively mentioned in his book—were ingenious technical innovations in practical malaria control. These contributions include the introduction of Paris green as a mosquito larvicide and the adaptation of the precipitin test to the blood meal of the mosquito. Recently Dr. Barber participated in research on DDT, which is for the time being the last word in mosquito control.

Chronologically and scientifically, much has happened since Laveran. It is the chronological coincidence between Dr. Barber's unusual career and the career of modern malariology which gives this book its principal significance to the historian of medicine.

SAUL JARCHO

The Mount Sinai Hospital, New York

CARLOS MARTÍNEZ DURÁN. *Las Ciencias Médicas en Guatemala—Origen y Evolucion.* Guatemala: C. A. Tipografia Sanchez & De Guise, 1942. 440 pp.

PROFESSOR MARTÍNEZ DURÁN, teacher of pathology at the university of Guatemala and rector of the University, is one of the outstanding clinicians in Latin America who devote their studies to the history of medicine. In an interesting flourishing of historical research during the last decade, we have seen the rising interest in this study in all the Republics of Central and South America; history of medicine has been taught at many universities, important books and papers have been published, and medico-historical societies have been founded. There is an important contribution to the history of the evolution of medical thought and the development of practice in Guatemala, where the Spanish tradition played an important role and the influence of European and later of North American currents of thought were felt. These studies will offer the possibility of a complete and lively picture of the history of medicine in the western hemisphere.

The medicine of Guatemala is divided by Martínez Durán into six periods: medicine in the time before the conquest, medicine during the conquest, colonial medicine before the foundation of the University of San Carlos (1541-1681), colonial medicine from the beginning of the University to the end of the Spanish domination(1681-1821), and medicine during the independent life of the country.

Dr. Martínez Durán has devoted a very interesting chapter to the history of magic and primitive medicine of the Mayas, their mythology, and the anatomical and physiological knowledge of the Indians. The epidemics and especially plague were brought to the new world by the conqueror, or at

least began to assume epidemic form after the conquest. The reader will find many interesting notes and the original judgment of an expert pathologist about the present condition and the future of the Indians in Guatemala.

The sanitary and moral conditions at the time of the conquest, carried on by Pedro de Alvarado, were very bad. Wars and diseases had destroyed the populations. The conqueror brought with him neither physicians nor surgeons, and for many years no medical aid was available. Not until twenty-five years after the conquest was the first hospital founded in Almalonga. And medicine was practiced only by herbalists and monks without any scientific knowledge.

In the second period when the city of Santiago was founded after the destruction of Almalonga by an inundation, the first Spanish physicians came to Guatemala. In 1542 a Spanish doctor, Juan de los Rios, surgeon, physician, and pharmacist, began to practice medicine. Surgery was practiced also by barbers, some of whom enjoyed the greatest reputation. The Spanish missionaries performed an important service in the administration of the hospitals.

Medicine was then largely in the hands of ecclesiastics. In 1562 the first stone of the University was laid with the foundation of a college which later developed into the Royal and Pontifical University of San Carlos. The University was inaugurated on January 7, 1681, in a very solemn ceremony; seventy students of medicine were inscribed. The University then began to play an important role in the history of Central American medicine. The first professor of medicine was Nicolas de Suza, who was succeeded by Miguel Fernandez. The first degree in medicine was conferred in 1703. The accomplishments of Esparrogasa and of Pedro Molina especially in the field of colonial medicine are accurately described. The latter became *protomedico* of the state, was in charge of public health while professor of surgery. He was the last of the great colonial physicians.

During the independent life of Guatemala we follow in the pages of this interesting book the story of the San Carlos University: the introduction of antisepsis and asepsis, the experiment of Dario Gonzalez, the evolution of the medical school in the last decades. A splendid and brilliant activity marks the progress of the University in recent times.

The reader will find in the attractive book of which I have tried to give a short summary a rich bibliography and a glossary of the most usual Indian anatomical and pathological terms. This extensive work also contains the biographies of many of the great physicians of Guatemala, of almost all the professors of medicine, and many brilliant pages on the life of the students. The organization of the hospitals, the adventures of surgeons and physicians in contest with wonder healers, the foundation of the national College of Surgery find place in this book.

The history of medicine in Guatemala will surely find many attentive readers beyond the frontiers of the country which boasts of the San Carlos University as one of its most precious treasures. It gives an interesting and fascinating picture of medicine under peculiarly difficult circumstances; it stresses the courage and the activity of men who fought for the ideal of freedom and of science. The author of this book to whom we owe many important medical historical works will surely have a great influence on the future development of the University and on the progress of medicine in his country.

ARTURO CASTIGLIONI

Yale University

HEDVIG LIDFORSS STRÖMGREN. *Die Zahnheilkunde im neunzehnten Jahrhundert.* Copenhagen: Ejnar Munksgaard, 1945. 274 pp., illustrated. 20. d.Kr.

KARL SUDHOFF'S *Geschichte der Zahnheilkunde,* which appeared twenty-five years ago, and is still unsurpassed, deals with the development of this special branch of medicine only up to the beginning of the nineteenth century. Sudhoff was of the opinion that a detailed history of dentistry in recent times ought not be written by a man like himself who was a medical historian with general medical training, "but was rather an honorable task for the *living representatives of scientific dentistry,* which in the greatness of its achievements during this period certainly is the equal of any other branch of medicine. To write this history is the exclusive task of the scientific representatives of this field."

Twenty-five years elapsed before Sudhoff's demand found its fulfillment with the publication of *Die Zahnheilkunde im neunzehnten Jahrhundert* by the Danish dentist Hedvig Lidforss Strömgren.

Her book, written in German, can by no means be regarded as a continuation of Sudhoff's volume. The somewhat patronizing tone, as well as the division according to the various special branches of dentistry (which is indeed quite appropriate and useful), correspond neither to the style nor the arrangement of Sudhoff's work. Dr. Strömgren has produced a book, however, which as a continuation of her *Zahnheilkunde im achtzehnten Jahrhundert,* gives both the medical historian as well as the non-specialist an excellent picture of the development of dentistry during the last century.

Almost as if she were offering a dental digest of the nineteenth century, Dr. Strömgren presents a large number of important and interesting extracts from works of physicians and dentists of that period, who either made important contributions to the development of dentistry, or who in their statements reflect the spirit of the times. This feeling for the contemporary atmosphere which she wishes to impress on the reader is enhanced still

further by the accompanying 163 well-selected illustrations, most of them also taken from the original works which she cites.

The author correctly emphasizes that the development of dentistry to a high scientific and technical level in the course of the nineteenth century is due primarily to the introduction of anesthesia, antisepsis, and asepsis, but above all to the construction in 1868 of the foot type engine which replaced the primitive and unreliable *hand* drill that had been in use until then.

The improvement and factory manufacture of porcelain teeth, which had been produced for the first time in 1776, gradually led to the abandonment of the use, for dental prosthesis, of natural animal and human teeth which had been employed previously and continued to be used as late as the first quarter of the nineteenth century. Likewise, the introduction of vulcanized rubber by Charles Goodyear in 1855 as a basis for artificial teeth brought dental prostheses, which had been very expensive until then, within reach of the more modestly situated elements of the population.

In the chapter on surgery, in which the improvement of instruments for dental extraction during the last century is described, Dr. Strömgren gives an incorrect account of the action of the "favorite instrument of the eighteenth century, the so-called 'Pelican,'" when she says that with this instrument "a blow was delivered to the tooth to be extracted, which loosened it." The actual action of this instrument, which is no longer used because of its dangerous character, consists not in the delivery of a *blow,* but on the contrary in an extraordinarily strong *centrifugal pull,* which drives the tooth outwards, out of the socket, toward the buccal wall of the mouth.

Separate chapters discuss in detail dental prosthesis, the conservation of the teeth, the history of the drill, efforts to control pain, the development of the representation of dental structure, various theories on the origin of caries, paradentosis, antisepsis, care of the teeth in school children, as well as dental education, professional societies and organizations, and dental journalism in the nineteenth century. A bibliography which is not too detailed and a name and subject index supplement the contents of this interesting and instructive work.

CURT PROSKAUER

New York City

BERNHARD J. STERN. *Medicine in Industry.* New York: The Commonwealth Fund, 1946. xiv+209 pp. $1.50.

*Medicine in Industry* is written by a doctor of philosophy, who is a lecturer in sociology at Columbia University, and published by the Commonwealth Fund, as a part of the study carried out by the Committee on Medicine and the Changing Order established by the Council of the New York Academy of

Medicine. An estimate of the value of this report should consider the instructions of the Council to the Committee to "review the nature, quality, and direction of the economic and social changes that are taking place now and that are to be anticipated in the immediate future." Publication does not imply the Committee's endorsement.

A preface is contributed by Paul Reznikoff, M.D., who points out that the growth of industrialization in modern times has profoundly affected the lives of vast masses of people, that new hazards have been created, and that emotional problems have been multiplied. On the other hand, growth of industrialization has contributed to a general rise in the standard of living and has extended the scope of medical care. It should be noted that the statement, "because it has become widely accepted that the health and happiness of the worker's family have a direct influence on his efficiency, industries in increasing numbers are including the dependents of employees in their health plans," refers to a very limited practice, which has not received the approval of any official body of industrial physicians, and is advocated by very few. There is, however, a trend in this direction.

The monograph opens with a chapter entitled "Scientific Development in Industrial Medicine," which might more accurately be called the "History of Occupational Health," since the chapter consists entirely of references to earlier writers on occupational health problems. The sociological thinking on housing, nutrition, and other off-the-job factors influencing the worker is admirable, and the same can be said for the succinct form of highlighting many of the milestones in the progress of occupational health.

Under the title "Social and Legislative Background," industrial society is contrasted with pre-industrial society, and progress is traced historically, providing an excellent picture of the effects of mass production and urbanization upon people's health and the efforts which are being made to meet these changes. Reference is made to the problem of women in industry, to the importance of safety, and to medical service in small plants. The extent of industrial and occupational disability is well analyzed and there are detailed discussions of the handicapped worker in industry. Well documented proof is given refuting the view that the handicapped worker is prone to injury; the similarity of occupational rehabilitation to military reconditioning is shown.

Considerable space is devoted to the preventive services, and a plea is expressed for closer cooperation between industrialists and private and governmental agencies which are equipped to be of service.

A chapter is devoted to medical care and health insurance. Various plans are discussed, such as programs sponsored by industry, services offered by trade unions, insurance through commercial carriers, the Blue Cross Plan, Sick

Benefit Societies, and the Rhode Island Compulsory State Disability Insurance.

In closing, the industrial physician receives attention. It is pointed out that in spite of increased interest, and, as previously indicated, an enlargement of the field, the number of full time physicians in industry continues small (2300 in 1942). Studies have shown that the more recent graduates are more interested in industrial medicine than the older medical men. The duties of the industrial physician are discussed, some consideration is given to pre-employment examinations, and mention is made of the medical association devoted to the specialty.

There is an appendix listing and describing the industrial prepayment medical plans of companies employing large medical staffs. Each chapter is followed by a comprehensive bibliography.

A great deal of information is compressed into this book of two hundred and nine pages. The material is presented in a factual way and should act as a stimulus to further study in the many problems presented. *Medicine in Industry* should be read by every physician interested in occupational health; it should be in the medical departments in industry, and will be found of interest by management as well.

ALFRED WHITTAKER

Detroit, Michigan

CAREY P. MCCORD. *A Blind Hog's Acorns.* Chicago: Cloud Inc., 1945. 311 pp. $2.75.

THREE decades ago the field of industrial medicine was still *terra incognita* to the medical profession of this country. Indifference and ignorance of workers' diseases often went hand in hand with scorn for the few physicians who worked actively in that field. Alice Hamilton tells us that "for a surgeon or physician to accept a position with a manufacturing company was to earn the contempt of his colleagues as a 'contract doctor'. . . ." Confirmation of this state of affairs is provided by Dr. McCord in the volume under review. "In early days," he says, "more of ire than tolerance was held for physicians who aligned themselves with industry for the purposes of the protection of the health of workers and who called themselves 'industrial physicians.' Privately such physicians were sometimes labeled 'renegades,' 'degenerates.' For use in public there was created the disapprobative term 'contract physician.' This term had a little nastiness in it and would be used in public with just the proper inflection to indicate that much more might be said on the subject did not courtesy intervene. In those days to enter the field of industrial health conservation carried with it the penalty of being shorn of a few of the outer garments of medical respectability."

Nevertheless, Dr. McCord "without any martyrdom . . . gaily accepted the badge of professional degradation." In 1919, he became an industrial physician. And in the present absorbing narrative, he leads the reader on a beguiling tour through twenty-five years of industrial hygiene. This pleasant and breezy autobiography travels a bizarre gamut of occupations and their effects on health, and the telling is generously interlarded with asides on the curious mental and emotional quirks of the human animal. At times the experiences related by Dr. McCord are hilariously comical, as, for instance, the account of the airplane trip in the chapter "Jungle Ulcer"; others are more somber.

Dr. McCord has spent a lifetime in the investigation of occupational disease, and he is well aware of the potentialities of industrial hygiene for the prevention and control of such conditions. Throughout the book, he stresses this point, sometimes directly, more often by implication through experiences that are not easily forgotten. As an introduction to an absorbing field of medical activity, Dr. McCord's volume is heartily recommended. It will undoubtedly be read with great interest by professional and layman alike.

GEORGE ROSEN

R. H. GEOGHEGAN. *The Aleut Language.* Edited by Fredericka I. Martin. United States Department of the Interior, 1944.

THE WAR has made it superfluous to explain why an American book on the Aleut language is a vital necessity. And even the more esoteric reasons for our scientific interest in the aboriginal language of the island chain linking Asia and North America are now understood so widely that further explanations seem unnecessary.

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The University of Wisconsin ERWIN H. ACKERKNECHT

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